PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCVI.

PART I.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCVI.

Jos: Banks

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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LIST

OF

THE ROYAL SOCIETY,

FOR THE YEAR 1797.

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THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1795, the Medal on Sir GODFREY COPLEY's Donation, to Mr. JESSE RAMSDEN, F. R. S. for his various inventions and improvements in the construction of the instruments for the trigonometrical measurements, carried on by the late Major General ROY, and by Lieutenant Colonel WILLIAMS and his Associates.

PHILOSOPHICAL TRANSACTIONS.

I. *The* Croonian *Lecture on Muscular Motion. By* Everard Home, *Esq. F. R. S.*

Read November 12, 1795.

In the Croonian Lecture which I had the honour of laying before this learned Society last year, I endeavoured to prove, that the adjustment of the eye to different distances could take place independent of the crystalline lens; and when this was the case, it appeared to arise from a change in the curvature of the cornea.

I propose in the present lecture to prosecute the inquiry; and it will be found in this, as well as in the former, that I have received the most essential assistance from Mr. Ramsden, who continues to interest himself in the investigation, and has made all the optical experiments.

As this was a new mode of explaining the adjustment of the eye, and differed from the theories that have been previously formed upon the subject, it was thought right to consider it with caution, to pay attention to all the objections that could

be made to it, and to put it to the test of such experiments as appeared likely to refute or confirm our former observations.

It readily suggested itself, that if the convexity of the cornea was increased to a certain degree, it could be measured by means of an image reflected from its surface, and viewed in an achromatic microscope with a divided eye-glass micrometer.

To ascertain whether the quantity of increase of the convexity of the cornea, in the adjustment of the eye, could in this way be ascertained, the following experiments were contrived, and made by Mr. RAMSDEN.

Our former experiments had sufficiently proved the unsteadiness of the human eye; the first trials on the present occasion were therefore made upon convex mirrors, as these artificial corneas could be more readily managed, and such previous experiments would enable us to apply the same instruments with more facility to the eye itself.

Two convex mirrors, one $\frac{4}{10}$ of an inch focus, the other $\frac{5}{10}$, had their flat surfaces made rough, and blacked, to prevent an image being seen from both surfaces, which was found to be the case when this precaution was omitted. One of these mirrors was stuck upon a piece of wood directly opposite to a window, at twelve feet distance from it; a board, three feet long, and six inches broad, was placed perpendicularly against the sash of the window, and its image reflected from the mirror upon the object-glass of an achromatic microscope, with a divided eye-glass micrometer.

The two images were separated by means of the divided eye-glass, till their surface of contact, which appears like a black line, was rendered as small as possible. When this effect was produced on the images from the mirror of $\frac{4}{10}$ of an

inch focus, that mirror was removed, and the other put in its place; the contact of the two images, which before appeared like a line, had now acquired considerable breadth; corresponding exactly to the difference between the convexities of the mirrors.

Having in this way made trial of the instruments, and arranged all the necessary circumstances, the head of a person was so placed as to bring the eye into the same situation as the mirror, and made steady by the apparatus described in our former experiments. Under these circumstances the image reflected from the cornea was measured by the micrometer.

Mr. RAMSDEN made an experiment with this instrument upon my eye. In the first trials, when the eye was fresh, there was a perceptible change in the micrometer, but extremely small; this was not, however, seen afterwards, and the eye very soon became so much fatigued that it was necessary to desist. He found that every time the eye adapted itself to different distances, it was necessary to move the object-glass of the microscope further from, or nearer to, the cornea.

This experiment was repeated on four different days; and in each experiment, on the first trial, the result was a change in the micrometer, but in all the subsequent trials it could not be detected. We were induced to conclude, that the effect on the micrometer might arise from the head being moved forwards, as we found, in making experiments with the mirror, that this effect could be produced by such motion; but had it arisen from that cause, it should more frequently have occurred, and rather after the head and eye were tired, than on the first trials. It was supposed to arise from the action of the muscles of the head, but that should have produced a contrary

appearance. The effect produced on the micrometer, therefore, did not seem to depend upon external circumstances, but to arise from a change in the cornea; it was, however, too small to admit of any conclusions being drawn from it.

The same experiment was made upon several young persons; but we found it necessary, that whoever was the subject of the experiment should understand perfectly what was meant to be done, otherwise the conclusions could not be depended on; for if the eye does not see the near object with a very defined outline, it is not accurately adjusted to it; and the length of time they kept their eye upon the near object without making any complaint of being fatigued, was greater, we knew, from our own observation, than it was possible to do it, had the object been seen with the necessary degree of distinctness.

I have to regret that Sir HENRY ENGLEFIELD, who took a part in the former experiments, and whose assistance in making these would have been of material advantage, was unable to remain in town.

Finding from these experiments, that the change in the convexity of the cornea was not to be seen distinctly in the micrometer, it became an object to ascertain the degree of change which could in this way be distinctly determined.

For this purpose two mirrors were ground, and prepared in the same way as those used in the preceding experiment; their radii were exactly ascertained by measuring the tools in which they were finished off; the one was $\frac{4}{10}$ of an inch focus, the other $\frac{408}{1000}$; the difference between the size of the images reflected from their surface was just visible in the micrometer; and from their remaining fixed, the experiment could be made with every advantage; but it did not appear

probable that the same difference would have been visible had the mirror not been perfectly at rest. A smaller change could not therefore be detected in the eye; and when we consider the disadvantages under which an experiment of this nature must be made upon the human eye, from the unsteadiness of that organ, the short time it remains adjusted (a part of which is lost in bringing it within the focus of the microscope), and also from the motions of the head; it is not unreasonable to suppose that a change might take place in the cornea, to the same extent, without being distinctly seen.

To give an idea of the short time that a part can remain nicely adjusted by muscular action, I shall point out an experiment which any one may make upon himself: let him take a glass spirit level, and rest one end of it on a table, supporting the other with his hand, and endeavour to keep the air bubble in the middle; if the hand is very steady the bubble may be kept nearly in its place, but not exactly so, it will undulate, its motion corresponding with the actions of the muscles; making up for want of steadiness by short motions in contrary directions.

From these experiments the change in the curvature of the cornea could not be more than $\frac{1}{125}$ part of an inch, as any greater quantity would probably have been distinctly seen in the micrometer; this, however, is still more than was ascertained by our former experiments, which made it to exceed $\frac{1}{800}$ part of an inch.

This change in the cornea, on the first view of the subject, appeared sufficient to account for the adjustment of the eye, and when the lens is removed it probably may be sufficient; but the refractions at the cornea are so much changed by those at the

lens, as considerably to lessen their effect in fitting the eye for seeing near objects, and make this small increase of convexity inadequate to such an effect.

Finding this to be the case, it became necessary to examine the eye with attention, to see in what way the full effect was most likely to be produced. For this purpose the following experiments were made upon the human eye, to determine whether the axis of vision could be elongated by any uniform pressure applied to its coats.

The experiments were made in the following manner: an eye of a dead subject was carefully removed from the socket, before any change could be produced in consequence of death; and its different diameters were measured by a pair of calliper compasses. As soon as these were determined, a hole was made in the centre of the optic nerve, and a pipe fixed into it, through which air could be thrown into the cavity of the eye, so as to distend its coats. While distended in a moderate degree, by compressing with the hand a small bladder, containing air and quicksilver, attached to the pipe, the same diameters were measured again, and compared with those which were taken while in the natural state.

These experiments were made by Mr. MUTTLEBURY and Mr. WILLIAMS, two very intelligent and diligent students in surgery, who were filling situations that gave opportunity of making such experiments. They measured the diameters in these two states, and marked them on paper, without ascertaining their difference, so that there could be no fallacy in the measurement from any preconceived opinion; and I have every reason to believe there was none from inattention.

		Transverse diameter.	Axis from optic nerve.	Axis of vision.
		Twentieth parts of an inch.	Twentieth parts of an inch.	Twentieth parts of an inch.
The eye of a boy 6 years old, 45 minutes after death - -	Natural state	17 $\frac{1}{2}$	17 $\frac{1}{2}$	17 $\frac{1}{2}$
	Distended state	17 $\frac{1}{4}$+	17 $\frac{1}{4}$+	18
The eye of a man 25 years old, 1 hour after death - -	Natural state	17 $\frac{3}{4}$	17 $\frac{3}{4}$	17
	Distended state	17 $\frac{1}{2}$	17 $\frac{1}{2}$	17 $\frac{1}{2}$
The eye of a man 50 years old, 20 minutes after death - -	Natural state	19	19	18 $\frac{1}{2}$
	Distended state	19	19	18 $\frac{1}{2}$

From these experiments it appears, that the diameters of the eye do not always bear the same proportion; sometimes the transverse diameter is the longest, in other eyes it is of the same length as the axis of vision; but when the coats are distended, the transverse diameter is diminished, and the axis of vision is lengthened.

This change, however, does not take place at all ages, for at 50 it was not met with.

In these experiments the pressure was made in the most unfavourable way for producing the greatest degree of elongation in the axis of vision; it was, however, the least exceptionable mode for ascertaining that such an effect could take place; when the pressure is made laterally and from without, the elongation must be still greater; and the action of the

straight muscles is the most advantageous that could be imagined for that purpose.

This lateral pressure will not only elongate the eye, and increase the convexity of the cornea, but it will produce an effect upon the crystalline lens and ciliary processes, pushing them forward in the same proportion as the cornea is stretched. This is necessary for two reasons; *viz.* to preserve the cavity containing the aqueous humour always of the same size, and to keep the cornea and lens at the same distance from each other. The ciliary processes, as they form a complete septum between the vitreous and aqueous humours, must be moved forward, together with the lens, when the cornea is rendered more convex, and when the cornea recovers itself they are thrown back into their former situation. In order to effect this with the nicety that is required, the ciliary processes are probably possessed of a muscular power.

That the ciliary processes are muscular is a very generally received opinion, and in the course of this lecture I shall adduce some facts in favour of it; they will also tend to confirm the opinion of these processes being a sling, in which the lens is suspended, and rendered capable of a small degree of motion.

The result of this inquiry, which has not been confined to the support of any particular theory, but carried on with the sole view of discovering the truth, appears to be, that the adjustment of the eye is produced by three different changes in that organ; an increase of curvature in the cornea, an elongation of the axis of vision, and a motion of the crystalline lens. These changes in a great measure depend upon the contraction of the four straight muscles of the eye.

Mr. RAMSDEN has been good enough to make a computa-

tion, by which the degree of adjustment produced by each of these changes may be ascertained. This he has promised to render more correct; and also to institute a series of experiments by which the effects of the motion of the lens may be more accurately determined. From Mr. RAMSDEN's computation, the increase of curvature of the cornea appears capable of producing one-third of the effect; and the change of place of the lens, and elongation of the axis of vision, sufficiently account for the other two-thirds of the quantity of adjustment necessary to make up the whole.

Having explained the mode by which the axis of vision can be elongated, and the convexity of the cornea increased, in the human eye, for the purpose of its adjustment, I was desirous of applying these observations to the eyes of other animals, that I might see whether their different structures would admit of the necessary changes, for producing an adjustment to different distances in the same way.

As many animals are known to have their vision distinct at very different distances, it appeared that much information might be gained by examining the structure of the eyes of those whose range of vision varies most from that of the human eye.

Quadrupeds in general must have their eyes fitted to see very near objects, as many of them collect their food with their mouths, in which action the objects are brought very close to the eye. Birds are under the same circumstances in a still greater degree with respect to their food; but from their mode of life, they also require the power of seeing objects at a great distance. Fishes, from the nature of the medium in which they live, must have some other mode of adjusting the eye,

than that of a change in the cornea, as that substance is possessed of the same refractive power with the surrounding fluid.

To avoid confusion in so extensive a field of inquiry, I shall separately consider the peculiarities in the eyes of these different classes of animals, so far as they appear to be concerned in producing the adjustment to different distances.

Quadrupeds have three modes of procuring their food; one by their fore-paws only, which they use like hands, as all the monkey tribe; the second, by their fore-paws and mouths, as the lion, and cat tribe; the third, by the mouth only, as all ruminating animals. These three different modes require the food being brought to different distances from the eye; and it is curious, that the muscles of the eye are different in all the three tribes.

In the monkey tribe, the muscles of the eye are exactly the same as in the human. In the lion tribe, they are double in number, and the four intermediate muscles are lost in the sclerotic coat, at a greater distance from the cornea than the others. In the ruminating tribe, there are four muscles, as in the human eye; but there is also a muscle surrounding the eyeball, attached to the bottom of the orbit, round the hole through which the optic nerve passes, and lost upon the sclerotic coat immediately before the broadest diameter of the globe of the eye; the upper portion of this muscle is rather the longest, its insertion being nearly in a circular line at right angles to the axis of vision, but not to the axis of the eye from the entrance of the optic nerve.

In quadrupeds in general, the ball of the eye is broader in proportion to its depth, than in the human subject; in the bull

the proportion is $1\frac{10}{16}$ inch to $1\frac{6}{16}$. The cornea is larger and more prominent; its real thickness is hardly to be determined, since, as well as that of the human eye, it readily imbibes moisture immediately after death. When dried, it is thinner than the sclerotic coat in the same state. In ruminating animals, it appears externally of an oval form; it is not, however, really so, the cornea itself being circular, as in other animals; but a portion of it is rendered opaque, by a membrane which covers its external surface, and produces an oval appearance. This circular form of cornea is necessary, that when it is stretched it may form a regular curve.

The ciliary processes, as in the human eye, are connected with the choroide coat; but they are larger, and are united at their origin with the iris.

This structure of the eye in quadrupeds, so far as it differs from that of the human eye, appears calculated to increase the power of adjusting it to see near objects, and from the mode of life which these animals pursue, such additional powers appear necesssary to enable them with ease to procure their food.

Birds in general procure their food by means of their beak; and the distance between the eye and the point of the beak is so small, that they must have a power of seeing very near objects. From living in air, and moving through it with great velocity, they require for their own defence, as well as to assist them in procuring food, a power of seeing at great distances.

That birds of prey see objects distinctly at a great distance appears to be proved by the following observations. In the year 1778, Mr. BABER and several other gentlemen were upon a hunting party in the island of Cassimbusar in Bengal, about

15 miles north of the city of Marshedabad; they killed a wild hog of an uncommon size, and left it upon the ground near their tent. About an hour after it was killed they were walking near the spot where it lay; the sky was perfectly clear, not a cloud to be seen, and a dark spot in the air at a great distance attracted their notice; it appeared gradually to increase in size, and moved directly towards them: as it advanced it proved to be a vulture, flying in a direct line to the dead animal, on which it alighted, and began to feed voraciously. In less than an hour, 70 other vultures came in all directions, some horizontally, but most of them from the upper regions of the air, in which a few minutes before nothing could be seen. Mr. BABER was so much struck with the circumstance at the moment, that he said to his friends, MILTON'S poetical description of the vulture, being lured to its prey by the smell, would not apply to what they had just seen.

VOLNEY, in his travels through Egypt, mentions a circumstance somewhat similar. He says, "the conspicuous situa-
"tion of Aleppo brings numbers of birds thither, and affords
"the curious a very singular amusement: if you go after din-
"ner on the terraces of the houses, and make a motion as if
"throwing bread, numerous flocks of birds will fly instantly
"around you, though at first you cannot discover one; but
"they are floating aloft in the air, and descend in a moment to
"seize in their flight the morsels of bread which the inhabi-
"tants frequently amuse themselves with throwing to them."*
This account of VOLNEY is confirmed by my friend Dr. RUSSEL, who has furnished me with an additional fact upon this sub-

* VOLNEY, English Translation, Vol. II. chap. 27, page 154.

ject. Dr. RUSSEL says, that the relation of VOLNEY is true; and that it is the amusement of the inhabitants, or rather of the Europeans, to allure birds by throwing up pieces of bread from the flat tops of the houses; these birds, to the best of his recollection, are the common gull (*larus canus* LINN.), which appear only at certain seasons.

But a fact more to the purpose of the present inquiry, is what Dr. RUSSEL remembers often to have heard asserted by the European sportsmen at Aleppo, and indeed sometimes observed himself; namely, that in the most serene weather, when not a speck could be seen in the sky, nor any object discovered in the horizon of an extensive plain, a dog or other animal killed by accident, or shot, and left behind by the sportsmen as they traversed the country, in the space of a few minutes was surrounded by birds, before invisible, either of the vulture tribe or the sea eagles (*ossifragus* LINN.). Whether these birds by vision were directed to their prey, or allured by scent, he would not undertake to pronounce, but the phænomenon occasioned wonder; and the more so, as there was not time for putrefaction to take place, which might be supposed to diffuse scent to a great distance.

The eyes of birds are larger in proportion than those of any other animal, the eye of a thrush being equal to that of a rabbit. They are also broader in proportion to their depth than in the quadruped; and the cornea is more prominent.

The cornea is very thin when examined immediately after death, and is at that time more elastic than afterwards. In the goose, it was stretched so as to be elongated $\frac{2}{20}$ of an inch, but in an hour afterwards it had become thicker, and less elastic. The cornea is not united to the sclerotic coat by the

terminating of one abruptly in the other; but the two edges are bevilled off, and laid over each other for nearly one-tenth of an inch in the eye of the goose, and more where the eye is larger. In the recent state, the thin edge of the cornea is readily torn off from the inner surface of the sclerotic coat to which it adheres, so as to show this mode of union. This circumstance was known to HALLER, and is particularly described in his works.

There is a bony rim surrounding the basis of the cornea in the eyes of birds, which is peculiar to this class of animals. It is made up of a number of different parts, very commonly 13 in number; some of these are lapped over each other, but some have an irregular union, one part passing before, and the other behind the bony scale next to it. This bony circle, thus made up, is not equally broad in its different parts; it is broadest where it covers the upper and outer part of the eye, and narrowest where it covers the cornea towards the inner canthus.

This bony rim does not give an origin to the cornea, as might appear to a superficial observer, but is a bony hoop laid over the junction between the sclerotic coat and cornea; and as the thin edge of the cornea passes within the sclerotic coat, the principal attachment of the bony rim must be to that coat. The bony rim is adapted to the surface upon which it lies; the greatest part of its breadth is firmly connected to the sclerotic coat; and where the cornea projects, the anterior edge of the rim is turned forwards to correspond with that projection; here the scales are extremely thin, they terminate in a fine edge, and admit of being forced a little asunder, to adapt them to the stretched state of the cornea; but no such effect

can be produced upon the posterior part of the rim, the different parts being too firmly connected to admit of any separation.

The structure of this bony rim differs in different birds. In the goose and turkey the scales are thin and weak; in the cassuary they are thicker; and in the eagle they are very strong. In the owl, they put on a very different appearance; they are 15 in number, $\frac{6}{10}$ of an inch long, and instead of being lapped over one another, as in other birds, they are united by indented sutures; each portion is broadest next the sclerotic coat, and narrowest towards the cornea, giving the bony rim a conical form.*

This structure in the owl's eye differs from that in other birds, the anterior edge not admitting of being dilated to correspond with the change of figure in the cornea; this purpose in the owl is answered by a circular elastic ligament firmly attached to the anterior edge of the bony rim, and lying upon the outside of the basis of the cornea; there is a similar ligament in other birds, but less conspicuous.

This bony rim in the eyes of birds is particularly noticed by HALLER; specimens of it, whole and in separate parts, are preserved in Mr. HUNTER's collection; it has been also described by Mr. SMITH, in a paper read before this Society: I shall, therefore, not dwell longer upon its structure, as it is not to my present purpose to take further notice of it than to explain its use respecting the adjustment of the eye, the subject of the present lecture.

The straight muscles of the eye in birds arise from the bottom of the bony orbit, as in the quadruped, and are firmly

* See the annexed plate.

attached to the posterior edge of the bony rim just described; they are four in number.

The ciliary processes are larger and longer in birds, than in other animals whose eyes are of the same size; they are evidently continued from the choroide coat, and adhere firmly to the capsula of the crystalline lens.

In the eyes of birds there is a substance which is peculiar to that class of animals, called the marsupium. It is a process composed of a corrugated vascular membrane attached to the centre of the retina, where the optic nerve terminates. Its origin is in a straight line, extending from the termination of the optic nerve to the lower part of the eye; in the turkey $\frac{5}{20}$ of an inch in length, and connected with the bottom of the eye by an elastic ligament about $\frac{1}{40}$ of an inch thick. The number of folds of which it is composed varies in different birds, from 5 to 15, or more; they are all of the same length, which in the turkey is about $\frac{4}{20}$ of an inch; they are covered with the nigrum pigmentum, and are attached anteriorly to the capsula of the crystalline lens, either immediately, as in the goose, or by intermediate membrane, as in the turkey.*

The structure of the marsupium is very similar to that of the ciliary processes, but stronger in all its parts, and like them it has a connection with the crystalline lens.

The connection between the marsupium and lens, in a natural state of the parts, is from its transparency invisible; but in the goose and cassuary, where the marsupium extends to the capsula of the lens, if the parts are coagulated in spirits, it becomes very apparent, and in these birds such a connection is generally allowed. In other birds, it is doubted by some, and

* *Vide* piate.

denied by others, who have written upon the subject. HALLER has taken some pains upon this point: he found, that by pulling the marsupium the motion was communicated to the lens, but he was unable to make out the mode of union; and all his attempts to coagulate the cells of the vitreous humour were unsuccessful; he says, no spirits can produce such a change. I have found, however, that, after the eye has remained a few days in rectified spirits, the medium between the marsupium and lens is coagulated, and rendered visible. By this means I have detected it in the turkey's eye; it is connected to the whole anterior extremity of the marsupium, extends to the capsula of the lens, aud appears to be about one half the length of the marsupium itself.

The union has been supposed to be extremely weak, because after death it readily gives way; this, however, is by no means the case, for when it is coagulated in rectified spirits, it is not easily torn; and the reason of its giving way in the dead eye, is probably from dissolution readily taking place when surrounded by moisture.

The anterior edge of the marsupium in some birds is narrower than its base, as in the cassuary; in others, it is of the same extent, as in the turkey; and in all, I believe, it is an uniform line; but when it is separated from the lens the folds contract irregularly, and appear of different lengths. In the eagle the marsupium is uncommonly strong.

From the similarity of structure in the marsupium and ciliary processes, as also their connection with the crystalline lens, I was desirous of ascertaining whether the marsupium was possessed of any muscular power, as this would determine

the same point with respect to the ciliary processes, and might lead to an explanation of the use of both these parts.

With this view I made the following experiments. The marsupium and crystalline lens of a goose's eye were exposed immediately after death; and the lens was pushed forwards, by which means the marsupium was elongated, and measured $\frac{5}{20}$ of an inch; upon taking off the pressure, it again contracted to $\frac{7}{40}$; this was repeated several times. The parts were then left, till it was supposed that all remains of life were gone, and the same experiment was repeated. In the stretched state it measured as before, $\frac{5}{20}$ of an inch, but in the contracted state, $\frac{4}{20}$; this change arose from the elasticity of the ligament connecting the marsupium to the bottom of the eye; and therefore the contraction of $\frac{3}{40}$, which was now lost, must have arisen from some other cause.

The result of this experiment favours the idea, that the marsupium possesses a muscular power, but in matters where we are so liable to be deceived, it seemed not a sufficient proof; I therefore made several other experiments, but they were all liable to some objections; the following, however, appears satisfactory, and shows that there is a power of contraction in the marsupium independent of elasticity.

The crystalline lens of a turkey's eye was extracted, and immediately afterwards the turkey was killed, by wounding the spinal marrow; the two eyes were taken out, and put into spirits.* In the one, the marsupium had nothing to pre-

* In the act of dying, the muscles are found to contract to their utmost, where there is no resistance to prevent such action; this is also found to take place in the greatest degree, when the animal is killed by any violence committed upon the brain, or spinal marrow.

vent its contracting to the utmost; while in the other, the lens being in its natural situation, could not allow of any unusual contraction. Some days after, the two eyes were examined; in the perfect eye the marsupium measured $\frac{4}{20}$ of an inch, and the different folds of it were semitransparent; in the imperfect eye the marsupium measured $\frac{3}{20}$ of an inch, and the folds were much more opaque. Here, then, was a difference of $\frac{1}{20}$ of an inch in the length of the two marsupia; which could arise from no other cause than the one having contracted so much more than the other, which contraction we must consider as muscular.

HALLER denies the marsupium to be muscular, because there is no such appearance in its structure. My own opinions upon the structure of muscles have been already explained to this learned Society; and I have lately met with an observation in LYONET's dissection of a caterpillar which tends to confirm them. He says, the muscles of the caterpillar are, in their natural state, transparent as jelly, and have vessels passing through their substance in every direction, which afford to the eye of the observer in the microscope the most beautiful appearance of a congeries of vessels.*

The peculiarities in the bird's eye are such as tend to facilitate both the lengthening of the axis of vision, and increasing the convexity of the cornea.

* "Les muscles des chenilles, dans leur état naturel, ils sont mous, ils prêtent extrêmement, ils ont la transparence d'une gelée, ils sont d'un gris bleuâtre, et les bronches argentées, ou vaisseaux aëriens, qu'on voit alors distinctement ramper par dessus, et penetrer dans toute leur substance, offrent à la loupe un spectacle qu'on ne se lasse point d'admirer."—*Traité Anatomique de la Chenille, par* PIERRE LYONET, chap. 6, page 92.

The bony rim, to which the muscles are attached, confines the effect of their pressure to the broadest part of the eye; and as their action throws forwards the cornea, the anterior edge of the bony rim yields, to adapt itself to that change; the ciliary processes are long, to admit of the lens being moved forwards, and by their action bring it back to its place; by these means the eyes of birds are adjusted to see very near objects with more facility than the eyes of other animals.

As the eyes of birds are likewise to be adjusted to see very distant objects, the marsupium is placed behind the crystalline lens, to draw it backwards, and when it acts, part of the pressure from behind being removed, the cornea is rendered flatter; and the anterior edge of the bony rim is adapted to it, in this state, by the contraction of the annular elastic ligament.

It may be said, that to see with parallel rays no such great change is necessary; it must, however, be considered that where vision is to be very distinct, a certain nicety of adjustment becomes necessary, and the action of the marsupium is probably intended for that purpose.

In the bird (although not immediately connected with the present subject) there is one of the most beautiful illustrations of the combination of muscular and elastic substances. This is employed for the motion of the membrana nictitans, and as it shews that such a combination is adopted wherever it can be used with advantage, and is provided as a defence for the organ in which I am endeavouring to explain such a combination, I cannot avoid taking notice of it. The membrana nictitans is composed of an elastic membrane, which is connected by means of a tendon, with two muscles situated

upon the posterior part of the eyeball; the action of these muscles brings the membrane over the cornea, and the instant they cease to contract, the elasticity of the membrane draws it back again.

The eyes of fishes have several peculiarities, and in many respects their structure differs from that which is observed in the quadruped and bird.

The muscles of thé eye, that correspond to the straight muscles in the quadruped, are four in number, they are, however, differently placed; they do not surround the eyeball; but two of them are on that side of the orbit next to the nose of the fish, the other two on the opposite side; their attachment to the eye is close to the edge of the cornea; they do not, however, pass round the eyeball towards the posterior part, as in other animals, but are connected with the bones of the head at some distance from the eye on each side; so that they cannot at all compress the eye laterally, they can only pull it backwards by the combined effect of their action.

The bottom of the orbit on which the eyeball rests, is solid, and adapted to it, there being no fat interposed between them as in other animals; and where the eye is removed to a great distance from the skull, and that cannot be the case, there is a strong cartilage projecting from the skull to the bottom of the eye, and that end of it next to the eye is concave, and fitted to the portion of the eyeball directly opposite the cornea, just above the entrance of the optic nerve. This is considered as a fixed point upon which the eye moves, but it will also, from the situation of the muscles, allow the eye to be forced back upon it, and the whole eye to be flattened.

The shape of the eye differs considerably in different fishes,

but in all of them the transverse diameter is the longest. In the haddock, the proportion is $\frac{11}{10}$ths to $\frac{8}{10}$ths of an inch, and in some fishes it differs much more.

The size of the eye does not correspond with that of the fish; the salmon's eye being smaller than the haddock's.

The sclerotic coat is in some fishes membranous;* in some partly bone,† in others entirely so,‡ but in general the posterior part is membranous, although the lateral parts are bone.§

The cornea is in general flat, not always circular in its shape, is very thin, made up of laminæ, and does not lose its transparency in spirits, appearing like talc.‖ In others it is more convex, as in fish of prey; this appears to adapt it to the spherical crystalline lens, which in them lies directly behind it.** The tunica conjunctiva forms the anterior layer of the cornea,†† and in some fishes is quite detached.

In the eel there is a transparent horny convex covering, at some distance before the eye, to defend it from external accidents. This covering, to an eye fitted to see in air, would entirely take off the effects arising from change of figure in the cornea; but in water, where no such change could be attended with advantage, such a covering is employed as an external defence.

In the eyes of fishes, the ciliary processes are entirely wanting. The crystalline lens is spherical, and imbedded in the vitreous humour, which is inclosed in cells of a stronger texture than in other animals.

The iris does not admit of motion; this is taken notice of by

* Haddock. † Sword-fish. ‡ Devil fish. § Mackerel.
‖ Sword-fish. ** Pike. †† Haddock.

HALLER; and the reason probably is, that the light in water is never too strong for the eye to bear.

There is a muscle situated between the retina and the sclerotic coat, which is, I believe, common to all fish. This muscle is particularly described by HALLER; and its use is stated to be that of bringing the retina nearer the crystalline lens, for the purpose of seeing objects at a greater distance. Mr. HUNTER called it the choroide muscle, and has preserved several preparations of it.

This muscle has a tendinous centre round the optic nerve, at which part it is attached to the sclerotic coat; the muscular fibres are short, and go off from the central tendon in all directions; the shape of the muscle is nearly that of a horse-shoe; anteriorly it is attached to the choroide coat, and by means of that to the sclerotic. Its action tends evidently to bring the retina forwards; and in general, the optic nerve in fishes makes a bend where it enters the eye, to admit of this motion without the nerve being stretched.

In those fishes that have the sclerotic coat completely covered with bone, the whole adjustment to great distances must be produced by the action of the choroide muscle; but in the others, which are by far the greater number, this effect will be much assisted by the action of the straight muscles pulling the eyeball against the socket, and compressing the posterior part; which, as it is the only membranous part in many fishes, would appear to be formed so for that purpose.

In fishes, the eye in its natural easy state appears to be adjusted to near objects, requiring some change to adapt it to see distant ones; in this respect differing entirely from the bird, the quadruped, and the human.

As the change which the eye is to undergo is different, so are the parts which produce it. The cornea, in many fishes, is neither circular, prominent, nor elastic, and the ciliary processes are wanting. The straight muscles pass off in different directions, to prevent the eye from being pressed upon laterally; the coats of the eye at that part are bony, in some fishes, to prevent the same effect; and the bottom of the orbit, which in other animals is filled with fat and loose cellular membrane, has no such covering, but is a hard concave surface, to give resistance, and assist in flattening the eye.

From the preceding observations, deduced from the structure of the eye in different animals, it appears that there are two modes of adjusting the eye, one for seeing in air, the other for seeing in water; and it is probably the want of this knowledge that has misled former inquirers, by confining their researches to the discovery of some one principle common to the eyes of all animals.

The crystalline lens, as the most conspicuous part, engrossed their whole attention, and they did not think any of the others capable of giving material assistance in producing so curious an effect.

The ciliary processes, from their connection with the lens, were by some believed capable of bringing it forwards; by others they were supposed to contract, and by that action elongate the eye, and remove the lens further from the retina: but these processes could never bring the lens forwards, unless the cornea was also moved forwards; for the lens and processes forming a complete septum, the aqueous humour would prevent the lens from making any advance in that direction: and the processes themselves are neither strong enough in

their muscular power, nor sufficiently attached to the coats of the eye, to alter its form by their contraction. In birds likewise, the bony rim renders this impossible.

That the axis of vision is really lengthened, and the lens moved forwards, for the purpose of adjusting the eye to see near objects, is rendered highly probable, since all the facts I have been able to collect seem to point out these changes; nor can the action of the external muscles increase the curvature of the cornea without producing them.

If the axis of vision being lengthened was believed by some physiologists to produce the whole adjustment of the eye to see near objects; if the crystalline lens being moved forwards was supposed by others to do the same thing; and if the cornea being rendered more convex appeared at the first view equally to account for it; all the three, when combined for that purpose, must undoubtedly be considered as sufficient to produce the effect.

EXPLANATION OF THE PLATE. (Tab. I.)

Fig. 1. A side view of the cornea of the eye of a goose, to show the bony rim, and elastic annular ligament, in their natural situation; *a* the bony rim; *b* the elastic ligament.

Fig. 2. A view of the same parts, in the eye of the great horned owl, to show the difference of structure; taken from a dried preparation in Mr. HUNTER's collection.*

* Since this lecture was read before the Royal Society, Sir JOSEPH BANKS has put into my hands a paper upon the anatomical structure of the eye, in which there is a plate, containing four views of the bony rim in the owl's eye. The parts they re-

Fig. 3. The marsupium in the eye of the turkey, attached to the bottom of the eye, and connected by a transparent membranous union with the crystalline lens; made visible by coagulation in rectified spirits.

Fig. 4. The marsupium in the eye of the emeu, from New South Wales, with a portion of the membrane that connects it to the lens; the marsupium is drawn together at that end next the lens, giving it the appearance of a purse, from which it probably got the name marsupium.

Fig. 5. and 6. Two views of the crystalline lens of the eye of a goose, to show the attachment of the marsupium to the lens.

These different drawings are of the natural size of the parts they represent.

present are exactly similar to those shown in the second figure; and had the paper been published in this country, would have rendered it unnecessary.

The paper is intituled *Esposizione Anatomica delle parti relative all' Encefalo degli Uccelli, del Sig.* VINCENZO MALACARNE; it is published in the Italian Transactions, called *Memorie di Matematica e Fisica della Società Italiana*, Tomo VII. Verona, 1794.

Fig. 1.

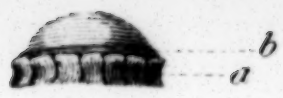


Fig. 2.

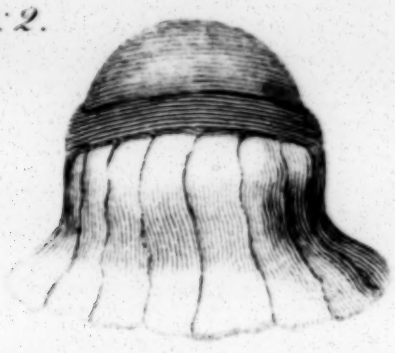


Fig. 3.

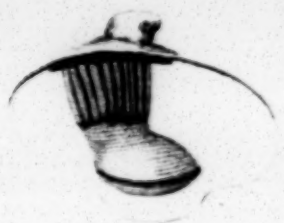


Fig. 4.

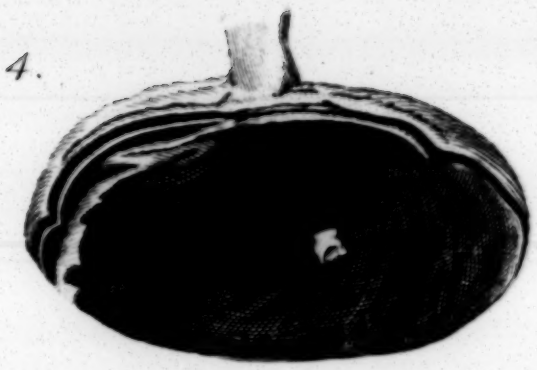


Fig. 5.



Fig. 6.



Bajire del. et Sc.

II. *Some Particulars in the Anatomy of a Whale.* *By Mr.* John Abernethy. *Communicated by* Everard Home, *Esq.* *F. R. S.*

Read November 26, 1795.

THERE are some particulars in the anatomy of the whale, which, I believe, have either entirely escaped observation, or have not been as yet communicated to the public. The parts which in the whale correspond in situation and office with the mesenteric glands of other animals, differ considerably from those glands in structure. These peculiarities are not only curious in themselves, but are illustrative of circumstances, hitherto esteemed obscure, in the anatomy and œconomy of the lymphatic glands in general. I therefore take the liberty of submitting the following account of them to the inspection of this learned Society.

The animal, from which the parts that I am going to describe were taken, was a male, of the genus named by LINNÆUS balæna.

Being desirous of making an anatomical preparation, to shew the distribution of the mesenteric vessels and lacteals of the whale, I procured for this purpose a broad portion of the mesentery with the annexed intestine; and proceeded in the first place to inject the blood vessels. The mesentery had been cut from the animal as close to the spine as possible:

had a less portion been taken away, the parts which I am about to describe would have been left with the body, for they are situated upon the origin of the blood vessels belonging to the intestines; and this, perhaps, is the reason why they have not been observed before.

When I threw a red-coloured waxen injection into the mesenteric artery, I saw it meandering in the ramifications of that vessel; but at the same time I observed it collecting in several separate heaps, about the root of the mesentery, which soon increased to the size of eggs. At the time, I imagined that the vessels had been ruptured, and that the injection in consequence had become extravasated; but I was conscious that no improper degree of force had been used in propelling the injection.

I next threw some yellow injection into the vein, when similar phænomena occurred; the branches of the vein were filled, but at the same time the masses of wax near the root of the mesentery were increased by a further effusion of the injection. These lumps had now acquired a spherical form, and some of them were of the size of an orange.

After the injection had become cold, I cut into the mesentery, in order to remove these balls of wax; when I found that they were contained in bags, in which I also observed a slimy and bloody-coloured fluid. On the inner surface of these bags a great number of small arteries and veins terminated; from the mouths of which the injection had poured into their cavities. There were seven of these bags in that piece of mesentery which I had to examine; but I am not able to determine what number belonged to the animal; for I do not know whether the portion of mesentery that I possessed

was complete. Having removed the injection from these bags, I observed on the inside of them a soft whitish substance, apparently containing a plexus of lacteal vessels. This substance entered the bags at that part of them which was nearest to the intestines, and went out at the part next to the spine. I now poured some quicksilver into those lacteals which appeared to lead to this soft substance: the quicksilver soon entered the vessels which were contained in it, and thus its nature was ascertained. A number of lacteals having entered one of these bags, were observed to communicate with each other, then again to separate, and form other vessels, which went out of the bag. It was some time before the quicksilver passed through the plexus of vessels contained in the first bag; but after having pervaded it, it passed on to a second bag, in which was concealed a similar plexus of lacteals. The quicksilver permeated these last vessels with much greater facility than it did the former, and quickly ran out of the large lacteals which were divided at the origin of the mesentery. Besides those absorbents which passed through the bags in the manner described, there were great numbers of others, which terminated by open orifices in every part of them. When quicksilver was poured into any of the lacteals, which were found near the sides of the bags, it immediately ran in a stream into their cavities. I introduced about a dozen bristles through as many lacteals, into different parts of two of these bags. These were doubtless few, in comparison to the whole number which terminated in them, but as the mesentery was fat, and the vessels were small, more could not easily be passed.

I afterwards stuffed two of the bags with horse-hair, dried them, and preserved them as an anatomical preparation. In

this state great numbers of arteries and veins, but chiefly of the former vessels, are seen terminating on their inside, in the same indistinct manner as the foramina Thebesii appear when the cavities of the heart are laid open: the bristles also render visible the termination of a certain number of lacteals. I examined the sides of these bags, which were moderately thick and firm; but I did not see any thing which, from its appearance, I could call a muscular structure.

From the circumstances that have been related, it appears, that in the whale there are two ways by which the chyle can pass from the intestines into the thoracic duct; one of these is through those lacteals, which pour the absorbed chyle into bags, in which it receives an addition of animal fluids. The other passage for the chyle is through those lacteals which form a plexus on the inside of the bags; through these vessels it passes with some difficulty, on account of their communications with each other; and it is conveyed by them to the thoracic duct, in the same state that it was when first imbibed from the intestines. The lacteals, which pour the chyle into the bags, are similar to those which terminate in the cells of the mesenteric glands of other animals: there is also an analogy between the distribution of the lacteals on the inside of these bags, and that which we sometimes observe on the outside of the lymphatic glands in general. In either case, a certain number of the vasa inferentia, as they are termed, communicate with one another, and with other vessels, named vasa efferentia.

By this communication, the progress of the fluids contained in these vessels is in some degree checked; which impediment increases the effusion into the cavities of the gland made by

the other lacteals: but should these cavities be obstructed, from disease, or other causes, an increased determination of fluids into the communicating absorbents must happen, which would overcome the resistance produced by their mutual inosculations, and the contents of the vessels would be driven forwards towards the trunk of the system. In the whale, as in other animals, we find that the impediment, occasioned by this communication of lacteals, is greatest in the first glands at which they arrive after having left the intestines.

The ready termination of so many arteries in the mesenteric glands of the whale, makes it appear probable, that there is a copious secretion of fluids mixed with the absorbed chyle; and, as I have before observed, a slimy bloody-coloured fluid was found in them. As the orifices of the veins were open, it appears probable that the contents of the bags might pass in some degree into those vessels.

The eminent anatomists, ALBINUS, MECKEL, HEWSON, and WRISBERG, were of opinion, that the lymphatic glands were not cellular, but were composed of convoluted absorbing vessels. This notion seems, however, to have been gradually declining.

Mr. CRUIKSHANK has of late publicly maintained a contrary opinion; and has shewn, that the cells of these glands have transverse communications with each other; which it is not likely they would have, if they were only the sections of convoluted vessels. Some additional observations have occurred to me, confirming this opinion, and which, as I believe they have not been publicly noticed by others, I beg leave to relate to this Society. I have injected the lymphatic glands of the groin and axilla of horses, with wax, and afterwards destroyed

the animal substance, by immersing them in muriatic acid. In some of these glands the wax appeared in very small portions, and irregularly conjoined; which is a convincing proof, that it had acquired this irregular form from having been impelled into numerous minute cells. But in several instances, I found one solid lump of wax, after the destruction of the animal substance: and it appears to me sufficiently clear, that the glands which were filled in this manner, were formed internally of one cavity, and were not, as is commonly the case, composed of many minute cells. I have also filled glands of this structure, in the mesentery of an horse, with quicksilver: I have then dried them, cut open the bags, and introduced a bristle into them through the vas inferens. And in the human mesentery, after having injected the artery, I have filled a bag resembling a gland, with quicksilver; which being opened, a mixture of injection and quicksilver was found in its cavity.

That the lymphatic glands in most animals are cellular, may not, perhaps, be hereafter doubted: that they are sometimes mere bags, analogy and actual observation induce me to believe. It might be said, that in those instances which I have related, the cells were burst, or that the glands were diseased: to which I can only reply, that there was no appearance to lead me to such a conclusion.

If, then, the lymphatic glands are either cellular, or receptacles resembling bags for the absorbed fluids, we are naturally led to inquire, what advantage arises from this temporary effusion of the contents of the absorbents. That there is a considerable quantity of fluids poured forth from the arteries of the whale, to mix with the absorbed chyle, is very

evident; nor can it be doubted that the same thing happens in other animals; for the cells of the lymphatic glands are easily inflated, and injected from the arteries.

The ready communication of these bags with the veins of the whale, induced me to examine whether I could ascertain any thing similar in other animals. Air impelled into the lymphatic glands, however, seldom gets into the veins; sometimes indeed veins are injected from these glands; but when this has occurred to me, I have observed an absorbent arising from the gland, and terminating in the adjacent vein.

These remarks, perhaps, may not be very important; such, however, is the nature of the subject, that all the knowledge we have hitherto obtained of the absorbing vessels has been acquired by fragments, and all our future acquisitions must be made in the same manner: I have wished, therefore, by offering these observations, to contribute my mite to the general stock of our knowledge of this subject.

III. *An Account of the late Discovery of Native Gold in* Ireland. *In a Letter from* John Lloyd, *Esq. F. R. S. to Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read November 19, 1795.

Cronbane Lodge, near Rathdrum,
the 4th of November, 1795.

DEAR SIR,

THE late very important mineralogical discovery in Ireland, and a desire I had long entertained of visiting the celebrated copper mine at this place, together with the opportunity that presented itself, of making my tour in company with our friend Mr. MILLS, who is one of the proprietors, as well as sole director of the mine, determined me to seize this moment for my excursion; and yesterday Mr. MILLS and I visited the spot, where so much pure gold has been of late taken up, being distant about 5 miles from this place.

About 7 miles westward of Arklow, in the county of Wicklow, there is a very high hill, perhaps 6 or 700 yards above the sea, called Croughan Kinshēlly, one of whose NE abutments, or buttresses, is called Balinnagore, to which the ascent may be made in half or three quarters of an hour. Should you have JACOB NEVILL's map of the county of Wicklow, published in 1760, at hand, by casting your eye on the river Ovo, which runs by Arklow, at about 4 miles above the latter place, you

will perceive the conflux of two considerable streams, and of a third about half a mile higher up, close to a bridge. By tracing this last to its source, you will come to a place, set down in the map Ballinvally; this is a ravine between two others, that run down the side of the hill into a semicircular, or more properly, semi-elliptical valley, which extends in breadth from one summit to the other of the boundary of the valley, and across the valley three-quarters of a mile, or somewhat less. The hollow side of the hill forms the termination of the valley, and down which run the three ravines abovementioned. At their junction, the brook assumes the name of Ballinasloge; at this place the descent is not very rapid, and so continues a hanging level for about a quarter of a mile, or somewhat more, when the valley grows narrower, and the sides of the brook become steeper; and it should seem, that some rocky bars across the course of the brook have formed the gravelly beds, above, over, and through which the stream flows, and in which the gold is found. The bed of the brook, and the adjacent banks of gravel, on each side, for near a quarter of a mile in length, and for 20 or 30 yards in breadth, have been entirely stirred and washed by the peasants of the country, who amounted to many hundreds, at work at a time, whilst they were permitted to search for the metal.

A gentleman, who saw them at work, told me, he counted above 300 women at one time, besides great numbers of men and children.

The stream runs down to the NE from the hill, which seems to consist of a mass of shistus and quartz; for on examination of the principal ravine, which is now washed clean by the late heavy rains, the bottom consisted of shistus, intersected at

different distances, and in various places, by veins of quartz, and of which substances the gravelly beds at the bottom, where the gold is found, seem to consist.

Large tumblers of quartz are thickly scattered over the surface of the top of the hill, under a turbary of considerable thickness, upon the removal of which these tumblers appear.

I shall not take up your time in attempting to give a minute geological description of this part of the country, as I have prevailed with Mr. MILLS (who from his minute examinations, and practical knowledge, is so conversant with the mineralogy of this county), to undertake that task, which I am persuaded he will perform to your satisfaction.

The gold has been found in masses of all sizes, from those of small grains to that of a piece of the weight of 5 ounces, which beautiful specimen is intended for the cabinet of a nobleman, adored in this country, and not less respected by his friends in England, and which, I dare to say, you will shortly have an opportunity of seeing in London. One piece of 22 ounces has been taken up, and which, I am told, is to be presented to his Majesty.

In our visit to this extraordinary place, we were most hospitably entertained by Mr. GRAHAM, of Ballycoage, whose house is not more than a mile from the gold mine: from him and his brothers I learnt, that about 25 years ago, or more, one DUNAGHOO, a schoolmaster, resident near the place, used frequently to entertain them with accounts of the richness of the valley in gold; and that this man used to go in the night, and break of day, to search for the treasure; and these gentlemen, with their schoolfellows, used to watch the old man in his excursions to the hill, to frighten him, deeming him to be

deranged in his intellects; however, the idea of this treasure did at last actually derange him.

JOHN BYRNE told me, that about 11 or 12 years ago, when he was a boy, he was fishing in this brook, and found a piece of gold, of a quarter of an ounce, which was sold in Dublin; but that, upon one of his brothers telling him it must have been dropped into the brook by accident, he gave over all thoughts of searching for more. CHARLES TOOLE, a miner at Cronbane, tells me, he heard of this discovery at the time, but gave no credit to it, as he never found any gold, and lives very near the place. I am credibly informed too, that a goldsmith in Dublin has, every year, for 11 or 12 years, bought 4 or 5 ounces of gold, brought constantly by the same person, but not JOHN BYRNE.

Thus, Sir, you have all I could learn respecting this important event; which is at your service to lay before the Royal Society, should you not have been furnished with an account from an abler pen.

I am, &c.

JOHN LLOYD.

P. S. I am told the name of the brook, where the gold is found, is, in Irish, *Aughatinavought*.

IV. *A mineralogical Account of the Native Gold lately discovered in* Ireland. *In a Letter from* Abraham Mills, *Esq. to Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read December 17, 1795.

Cronebane Copper Mines, near Rathdrum,
Nov. 21, 1795.

SIR,

THE extraordinary circumstance of native gold being found in this vicinity, early excited my attention, and led me to seize the first opportunity that presented itself, after my late arrival here, to inspect the place where the discovery was made.

I went thither on Tuesday, the 3d of this month, with Mr. LLOYD, of Havodŷnos, and Mr. WEAVER. The former having given you some account of the circumstances which attended the original discovery, and, since he left me, a favourable day having enabled me to take a second view of the adjacent country, I shall now attempt to describe the general appearance, and add such further information as has come to my knowledge.

The workings which the peasantry recently undertook, are on the north-east side of the mountain Croughan Kinshēlly, within the barony of Arklow, and county of Wicklow, on the lands of the Earl of CARYSFORT, wherein the Earl of ORMOND claims a right to the minerals, in consequence (as I have been informed), of a grant in the reign of King HENRY the Second,

by Prince JOHN, during his command of his father's forces in Ireland; which grant was renewed and confirmed by Queen ELIZABETH, and again by King CHARLES the Second.

The summit of the mountain is the boundary between the counties of Wicklow and Wexford; seven English miles west from Arklow, ten to the south-westward of Rathdrum, and six south-westerly from Cronebane mines; by estimation about six hundred yards above the level of the sea. It extends W by N and E by S, and stretches away to the north-eastward, to Ballycoage, where shafts have formerly been sunk, and some copper and magnetic iron ore has been found; and thence to the NE there extends a tract of mineral country, eight miles in length, running through the lands of Ballymurtagh, Ballygahan, Tigrony, Cronebane, Connery, and Kilmacoe, in all which veins of copper ore are found; and terminating at the slate quarry at Balnabarny.

On the highest part of the mountain are bare rocks, being a variety of argillite,* whose joints range NNE and SSW, hade to the SSW, and in one part include a rib of quartz, three inches wide, which follows the direction of the strata. Around the rocks, for some distance, is sound ground, covered with heath; descending to the eastward, there is springy ground, abounding with coarse grass; and below that, a very extensive bog, in which the turf is from four to nine feet thick, and beneath it, in the substratum of clay, are many angular fragments of quartz, containing chlorite, and ferruginous earth. Below the turbary the ground falls with a quick descent, and three ravines are observed. The central one, which is the most considerable, has been worn by torrents, which derive their source from the bog; the others are formed lower down

* KIRWAN, Edit. 1794, p. 234.

the mountain by springs, which uniting with the former, below their junction the gold has been found. The smaller have not water sufficient to wash away the incumbent clay, so as to lay bare the substratum; and their beds only contain gravel, consisting of quartz with chlorite, and other substances of which the mountain consists. The great ravine presents a more interesting aspect; the water in its descent has, in a very short distance from the bog, entirely carried off the clay, and considerably worn down the substrata of rock, which it has laid open to inspection.

Descending along the bed of the great ravine, whose general course is to the eastward, a yellow argillaceous shistus is first seen; the laminæ are much shattered, are very thin, have a slight hade to the SSW, and range ESE and WNW. Included within the shist, is a vein of compact barren quartz, about three feet wide, ranging NE and SW; below this is another vein, about nine inches wide, having the same range as the former, and hading to the northward, consisting of quartz, including ferruginous earth. Lower down, is a vein of a compact aggregate substance, apparently compounded of quartz, ochraceous earth, chert, minute particles of mica, and some little argillite, of unknown breadth, ranging E and W, hading fast to the southward, and including strings of quartz, from one to two inches thick, the quartz containing ferruginous earth. The yellow argillaceous shistus is again seen with its former hade and range; and then, adjacent to a quartz vein, is laminated blue argillaceous shistus, ranging NE and SW, and hading SE; which is afterwards seen varying its range and hade, running ENE and WSW, and hading NNW; lower down, the blue shist is observed more compact, though still

laminated. The ground, less steep, becomes springy, is inclosed, and the ravine, shallower, has deposited a considerable quantity of clay, sand, and gravel. Following the course of the ravine, or, as it may now more properly be called, the brook, arrive at the road which leads to Arklow; here is a ford, and the brook has the Irish name of *Aughatinavought* (the river that drowned the old man); hence it descends to the Aughrim river, just above its confluence with that from Rathdrum, which, after their junction, take the general name of the Ovo, that discharging itself into the sea near the town of Arklow, forms an harbour for vessels of small burthen.

The lands of Ballinvally are to the southward, and the lands of Ballinagore to the northward, of the ford, where the blue shistus rock, whose joints are nearly vertical, is seen ranging ENE and WSW, including small strings of quartz, which contain ferruginous earth. The same kind of earth is also seen in the quartz, contained in a vein from ten to twelve inches wide, ranging ENE and WSW, and hading to the southward, which has been laid open in forming the Arklow road.

Here the valley is from twenty to thirty yards in width, and is covered with substances washed down from the mountain, which on the sides have accumulated to the depth of about twelve feet. A thin stratum of vegetable soil lies uppermost; then clay, mingled with fine sand, composed of small particles of quartz, mica, and shist; beneath which the same substances are larger, and constitute a bed of gravel, that also contains nodules of fine grained iron stone, which produces 50 *per cent.* of crude iron: incumbent on the rock are large tumblers of quartz, a variety of argillite and shistus; many pieces of the quartz are perfectly pure, others are attached to the shistus,

others contain chlorite, pyrites, mica, and ferruginous earth; and the arsenical cubical pyrites frequently occurs, imbedded in the blue shistus. In this mass of matter, before the workings began, the brook had formed its channel down to the surface of the rock, and between six and seven feet wide, but in times of floods extended itself entirely over the valley.

Researches have been made for the gold, amidst the sand and gravel along the run of the brook, for near half a mile in length; but it is only about one hundred and fifty yards above, and about two hundred yards below the ford, that the trials have been attended with much success: within that space, the valley is tolerably level, and the banks of the brook have not more than five feet of sand and gravel above the rock; added to this, it takes a small turn to the southward, and, consequently, the rude surfaces of the shistus rock in some degree cross its course, and form natural impediments to the particles of gold being carried further down the stream, which still lower has a more rapid descent; besides, the rude manner in which the country people worked, seldom enabled them to penetrate to the rock, in those places where the sand and gravel were of any material depth. Their method was, to turn the course of the water wherever they deemed necessary, and then, with any instruments they could procure, to dig holes down to the rock, and by washing, in bowls and sieves, the sand and gravel they threw out, to separate the particles of gold which it contained; and from the slovenly and hasty way in which their operations were performed, much gold most probably escaped their search; and that indeed actually appears to have been the case, for since the late rains washed the clay and gravel which had been thrown up, gold has been

found lying on the surface. The situation of the place, and the constant command of water, do, however, very clearly point out the great facility with which the gold might be separated from the trash, by adopting the mode of working practised at the best managed tin stream works in the county of Cornwall; that is, entirely to remove (by machinery) the whole cover off the rock, and then wash it in proper buddles and sieves. And by thus continuing the operations, constantly advancing in the ravine towards the mountain, as long as gold should be found, the vein that forms its matrix might probably be laid bare.

The discovery was made public, and the workings began, early in the month of September last, and continued till the 15th of October, when a party of the Kildare militia arrived, and took possession by order of government; and the great concourse of people, who were busily engaged in endeavouring to procure a share of the treasure, immediately desisted from their labour, and peaceably retired.

Calculations have been made, that during the foregoing period, gold to the amount of three thousand pounds Irish sterling was sold to various persons; the average price was three pounds fifteen shillings *per* ounce; hence eight hundred ounces appear to have been collected within the short space of six weeks.

The gold is of a bright yellow colour, perfectly malleable; the specific gravity of an apparently clean piece 19,000. A specimen, assayed here by Mr. WEAVER, in the moist way, produced from 24 grains, $22\frac{58}{101}$ grains of pure gold, and $1\frac{43}{101}$ of silver. Some of the gold is intimately blended with, and adherent to quartz; some (it is said) was found united to the

fine grained iron stone, but the major part was entirely free from the matrix; every piece more or less rounded on the edges, of various weights, forms, and sizes, from the most minute particle up to 2*oz.* 17*dwt.*; only two pieces are known to have been found of superior weight, and one of those is 5, and the other 22 ounces.

I much regret not having been present when the work was going on, that I might have seen the gold as found, before prepared for sale by breaking off any extraneous matter that adhered; for in that state, a proper attention to the substances with which it was united, and a subsequent diligent inspection of the several veins that range through the mountain, might assist towards the discovery of that from whence it was detached.

I shall shortly return to England; and on my arrival, will send specimens of the gold, and of the different substances of the mountain, to be deposited (if you think proper) in the collection of the Royal Society.

And am, with great respect, &c.

ABRAHAM MILLS.

The bearings are all taken by the compass, without allowing for the variation.

BESIDES these accounts of the gold found in Ireland, the following information has been received on that subject.

WILLIAM MOLESWORTH, Esq. of Dublin, in a letter to RICHARD MOLESWORTH, Esq. F. R. S. writes, that he weighed the largest piece of gold in his balance, both in air and water;

that its weight was 20 *oz.* 2 *dwt.* 21 *gr.* and its specific gravity, to that of sterling gold, as 12 to 18. Also that RICHARD KIRWAN, Esq. F. R. S. found the specific gravity of another specimen to be as 13 to 18. Hence, as the gold was worth £ 4 an ounce, Mr. WILLIAM MOLESWORTH concludes, that the specimens are full of pores and cavities, which increase their bulk, and that there are some extraneous substances, such as dirt or clay, contained in those cavities.

This opinion was discovered to be well founded, by cutting through some of the small lumps.

STANESBY ALCHORNE, Esq. his Majesty's Assay-master at the Tower of London, assayed two specimens of this native gold. The first appeared to contain, in 24 carats,

21 $\frac{6}{8}$ of fine gold;

1 $\frac{7}{8}$ of fine silver;

$\frac{3}{8}$ of alloy, which seemed to be copper tinged with a little iron.

The second specimen differed only in holding 21 $\frac{5}{8}$ instead of 21 $\frac{6}{8}$ of fine gold.

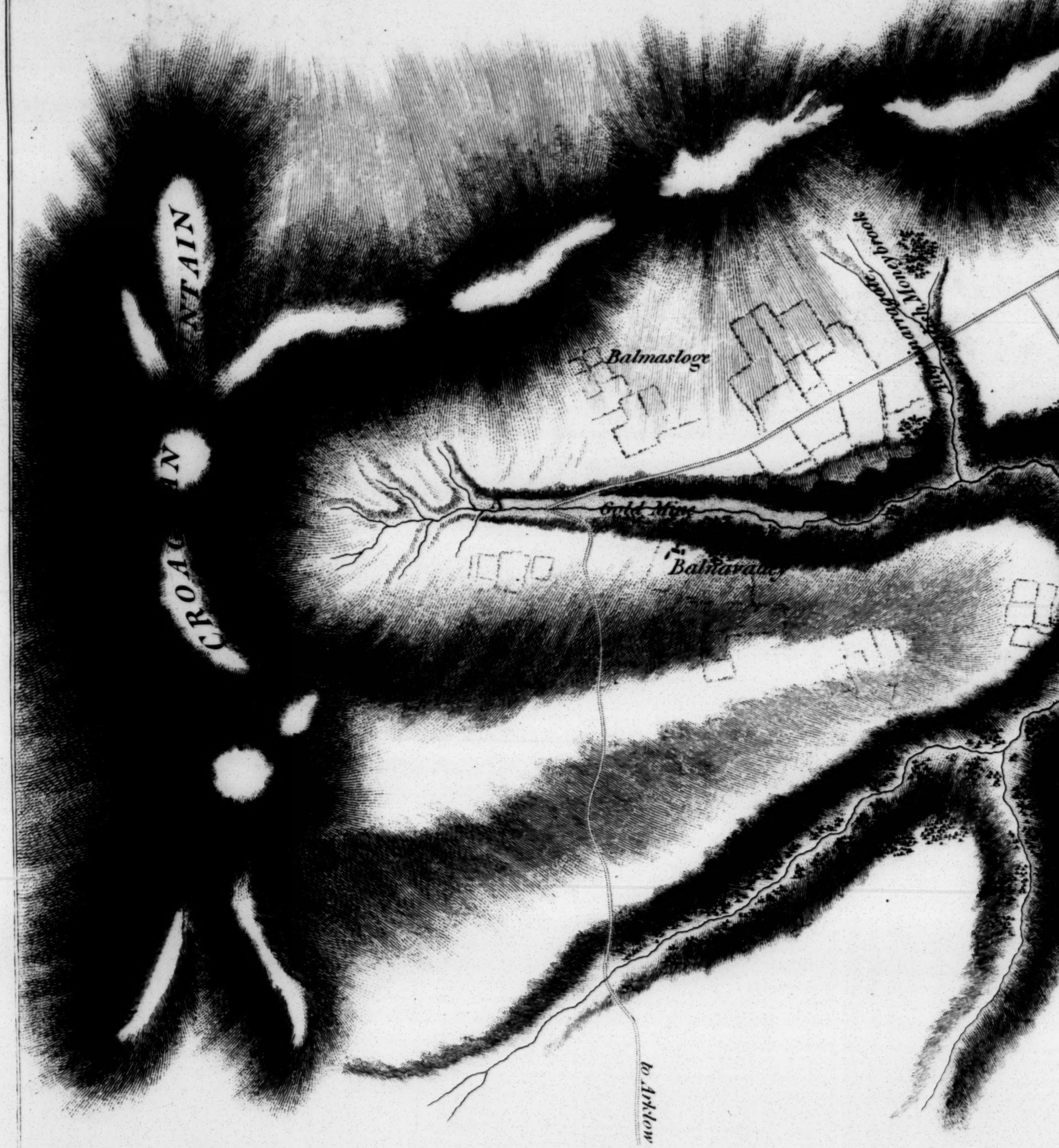
Major JOHN BROWN, of the royal engineers, transmitted to the Right Hon. THOMAS PELHAM a sketch of the spot where the gold was found, which Mr. PELHAM has obligingly permitted to be engraved, for the use of the Royal Society. See Tab. II.

C. B.

a piece of native gold weighing 48 ounces, 15 grains was brought from India by Mr. Hastings it was in the form of a Pebble

North

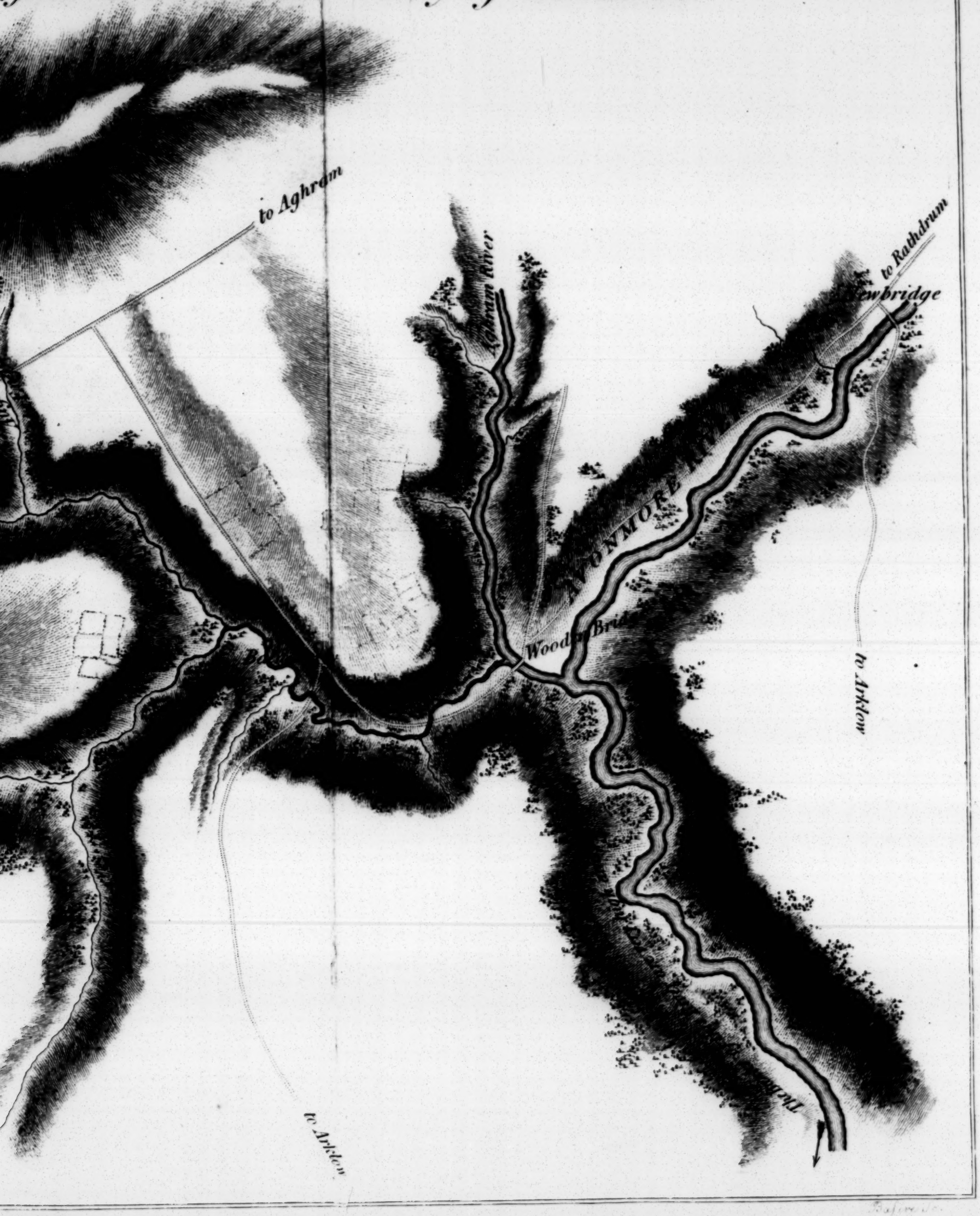
SKETCH of the GOLD MINE five Miles N.W. from A



South

North

from Arklow in the County of WICKLOW.



South

V. *The Construction and Analysis of geometrical Propositions, determining the Positions assumed by homogeneal Bodies which float freely, and at rest, on a Fluid's Surface; also determining the Stability of Ships, and of other floating Bodies.*

By George Atwood, *Esq. F. R. S.*

Read February 18, 1796.

To investigate the positions assumed by homogeneal bodies which float freely, and at rest, on a fluid's surface, it is necessary, in the first place, to form a just conception of the several principles on which those positions depend.

The proportion of the immersed part to the whole magnitude of a floating body* will always be obtained, from having given the specific gravity of the solid in respect to that of the fluid; since it is a known law of hydrostatics, that the immersed part of the solid is to the whole magnitude, in the proportion of those specific gravities. But a solid may be immersed in a fluid numberless different ways, so that the part immersed shall be to the whole magnitude in the given proportion of the specific gravities, and yet the solid shall not rest permanently in any of these positions. The reasons are obvious. The floating body is impelled downward by its weight, acting in the direction of a vertical line, which passes through the centre of gravity; the pressure of the fluid, by

* In these pages the floating bodies are always understood to be homogeneal, unless the contrary be mentioned.

which the solid is supported, acts upward, in the direction of a vertical line (usually called the line of support), which passes through the centre of gravity of the part immersed: unless, therefore, these two lines are coincident, so that the two centres of gravity shall be in the same vertical line, it is evident that the solid thus impelled, must revolve on an axis until it finds a position in which the equilibrium of floating will be permanent.

From these observations it appears, that to ascertain the positions in which a solid body floats permanently on the surface of a fluid, it is requisite that the specific gravity of the floating body should be known, in order to fix the proportion of the part immersed to the whole: secondly, it is necessary to determine, by geometrical or analytical methods, in what positions the solid can be placed on the surface of the fluid, so that the centre of gravity of the floating body, and that of the part immersed, may be situated in the same vertical line, while a given proportion of the whole volume is immersed under the fluid's surface.

These particulars having been determined, evidently reduce the statement of the problem into a narrow compass; but they are not alone sufficient to limit it: for although it has been shewn that a body cannot float permanently on a fluid unless the two centres of gravity, that have been mentioned, are situated in the same vertical line, it does not follow that, whenever those centres of gravity are so situated, the solid will float permanently in that position:* consistently

* Admitting any proposition to be true, the converse of the proposition may be either true generally, or with exceptions. To distinguish the cases in which it is true from those in which it fails, requires a separate demonstration or investigation.

with this observation, positions may be assigned, in which a solid is immersed in a fluid to the true depth according to its specific gravity, and the centre of gravity of the solid and that of the part immersed are in the same vertical line, yet the solid does not rest in any of these positions, but assumes some other in which it will continue permanently to float. To make this evident, a very obvious instance may be referred to. Suppose a cylinder, the specific gravity of which is to that of a fluid on which it floats as 3 to 4; and let the axis of the cylinder be to the diameter of the base as 2 to 1: if this cylinder is placed on the fluid with its axis vertical, it will sink to a depth equal to a diameter and a half of the base; and as long as the axis is sustained in a vertical position by external force, the centre of gravity of the solid, and the centre of the immersed part, will be situated in the same vertical line: but the solid will not float permanently in that position; for as soon as external support is removed, it falls from its upright position, and remains floating with the axis horizontal. If the axis of the cylinder is made only $\frac{1}{2}$ instead of twice the diameter of the base, the solid being placed with its axis vertical, will sink to the depth of $\frac{3}{8}$ of a diameter, and will float permanently in that position. Even if the axis should be placed not exactly coincident with the vertical, but in a direction somewhat inclined to that line, the solid will change its position until it settles permanently with the axis perpendicular to the horizon.

The cylinder here instanced is caused either to float permanently with its axis vertical, or to overset, according to the different proportions between the length of the axis and the diameter of the base: although an exact estimate of the effects

produced by altering these proportions, cannot be obtained except by mathematical investigation (a subject to be considered in some of the following pages), yet a general idea of the causes by which so remarkable a difference is occasioned in the floating position of the two cylinders, will appear obvious by attending to the changes which take place in the position of the line of support, while the solid is inclined from the upright through a small angle. For whenever the line of support, in the direction of which the force of the fluid's pressure acts, does not pass through the centre of gravity of the floating body, that force must generate a motion of rotation round an horizontal axis which passes through the centre of gravity of the solid; and must cause an elevation of those parts of the solid which are on the same side of the axis of motion with the line of support, and consequently must depress those parts which are situated on the contrary side of that axis. Admitting, therefore, that the solid is adjusted with its centre of gravity and the centre of the immersed part precisely to the same vertical line, and that a small inclination takes place round the axis of motion; it will depend on the position of the line of support, whether that inclination shall be counteracted, so as to restore the solid to its upright position, or shall be augmented; in which latter case the solid oversets. If the nature of the figure should be such as causes the line of support to be moved toward those parts which are immersed by the inclination, that inclination will be counteracted, because the pressure of the fluid generates angular motion in a direction contrary to that in which the solid is inclined; but if the figure is such as causes the line of support to be moved toward those parts of the solid which

are elevated by the inclination, the force of the fluid's pressure must continually augment the inclination; or, in other words, will cause the solid to overset, or change its position, until it settles in some other, in which the equilibrium is permanent.

We observe, therefore, that a solid floats permanently in a given position, only because the smallest inclination from that position creates a force by which the inclination is immediately counteracted, and the solid becomes restored to its upright position; and consequently, since the inclination is counteracted while of evanescent magnitude, no sensible deviation from the upright can take place: in cases of instability, the solid oversets, although placed on a fluid with the centre of gravity of the solid and that of the part immersed in the same vertical line, because the smallest deviation or inclination from that position creates a force by which the inclination is augmented. And since various causes concur in preventing the two centres from remaining adjusted to the vertical with a precision absolutely mathematical, it follows that the least or evanescent inclination here mentioned must necessarily subsist, and being continually augmented by the fluid's pressure, must become a sensible rotation, by which the solid oversets from its upright position.

In either case, that is, whether the solid floats permanently, or oversets, if it is placed on the surface of a fluid, so that the centre of gravity of the solid and the centre of gravity of the part immersed shall be in the same vertical line, the solid is said to be in a position of equilibrium: and from the preceding observations it appears, that there are three species of equilibrium in which a solid may be situated when the two centres of gravity just mentioned are in the same vertical line.

1st.* The equilibrium of stability, in which the solid floats permanently in a given position.

2dly. The equilibrium of instability, in which case the solid, although its centre of gravity and that of the part immersed are in the same vertical line, spontaneously oversets, unless sustained by external force. This kind of equilibrium is similar to that which subsists when a needle, or other sharp-pointed body, is placed vertically on a smooth horizontal surface.

3dly. The third species, being a limit between the two former, is called the equilibrium of indifference, or the insensible equilibrium, in which the solid rests on the fluid indifferent to motion, without tendency to right itself when inclined, or to incline itself further.

These different kinds of equilibrium may perhaps be more clearly perceived, by referring to the instance in which a cylinder was supposed to be placed on the surface of a fluid with the axis vertical. If the axis is assumed double the diameter of the base, the solid oversets, the equilibrium of position being that of instability: but if the length of the axis is only half the diameter of the base, the solid floats permanently with the axis vertical. It seems evident, therefore, that there must be some intermediate proportion between the cylinder's axis and the diameter of the base, greater than 1 to 2, and less than 2 to 1, which will correspond to the case intermediate, where stability ceases, and instability begins: this is the precise proportion when the equilibrium is of the species called the equilibrium of indifference, or the insensible equilibrium.

When a solid body floats permanently on the surface of a

* Euler. *Théorie complette de la Construction et Manœuvre des Vaisseaux*, chap. iv.

fluid, and external force is applied to incline it from its position, the resistance opposed to this inclination is termed the stability of floating. It is obvious to every one's experience, that some floating bodies are more easily inclined from their quiescent position than others; that, after having been inclined, some will return to their original situation with more force and celerity than others; a difference particularly observable in ships at sea, in some of which a given impulse of the wind will cause a much greater inclination from the perpendicular than in others. As this property of opposing resistance to heeling or pitching, when regulated to its due quantity and proportion, has been deemed of material consequence in the construction of vessels, several eminent mathematicians have been induced to investigate rules, by which the stability of ships may be inferred, independently of any reference to trial, from knowing their weights and dimensions only. It must, however, be acknowledged, that the theorems which have been given on this subject, in the works of Mons. BOUGUER,* EULER,† FRED. CHAPMAN,‡ and other writers, for determining the stability of ships, are founded on a supposition that the inclinations from their quiescent positions are evanescent, or, in a practical sense, very small. But as ships at sea are known to heel through angles of 10°, 20°, or even 30°, a doubt may arise how far the rules demonstrated on the express condition, that the angles of inclination are of evanescent magnitude, should be admitted as practically applicable in cases where the inclinations are so great.

* BOUGUER. Liv. i. sec. iii. chap. iv.

† EULER. *Théorie complette de la Construction et Manœuvre des Vaisseaux*, chap. iv. and chap. v.

‡ *Traité de la Construction des Vaisseaux par* FRED. CHAPMAN, chap. ii. p. 17.

To put this matter in a clear point of view, let a case be assumed. Suppose two vessels to be of the same weight and dimensions in every respect, except that the sides of one of these vessels shall project more than those of the other, the projections commencing from the line coincident with the water's surface. According to the theorems of BOUGUER and other writers, the stability will be the same in both ships, which is in fact true, on the supposition that their inclinations from the perpendicular are extremely small angles: but when the ships heel to 15° or 20°, the stabilities of the two vessels must evidently be very different. Even supposing the stability of a ship A to be greater than that of a ship B, when the angles of heeling are very small, it may happen in cases easily supposable that when both ships are heeled to a considerable angle of inclination, the stability of the ship B shall exceed that of the ship A. Admitting, therefore, that the theory of statics can be applied with any effect to the practice of naval architecture, it seems to be necessary that the rules investigated for determining the stability of vessels should be extended to those cases in which the angles of inclination are of any magnitude likely to occur in the practice of navigation.

When a solid is placed on the surface of a lighter fluid, at the proper depth corresponding to the relative gravities, it cannot change its position by the combined actions of its weight and the fluid's pressure, except by revolving on some horizontal axis which passes through the centre of gravity. Various axes may be drawn through the centre of gravity of a floating body in a direction parallel to the horizon: but since the motion of the solid respecting one axis only, can be the subject of the same investigation (except in extreme cases

not to be considered in this place), the figure of the floating body, and the particular object of inquiry, must determine to which of these axes the motion of the solid is to be referred, when it changes its position: thus, suppose a square beam of timber, the specific gravity of which is to that of water as 1 to 2, should be placed on the surface of that fluid with one of the flat surfaces parallel to the horizon (the length being assumed considerably greater than the breadth), no motion of rotation can take place round the transverse axis, by which the extremities of the beam would be elevated or depressed: but the solid will spontaneously revolve in this instance round the longer axis, changing its position until it settles with an angle upward.

In like manner, if the same solid should be placed horizontally on the surface of the water with an angle upward, it will not spontaneously change its position; but if one extremity of the beam should be forcibly elevated, and the other depressed, so as to incline the longer axis to the horizon, as soon as all external force is removed, the beam will revolve on a transverse horizontal axis, passing through the centre of gravity, and perpendicular to the longer axis, until it settles in such a position as to leave the longer axis horizontal. These are instances in which the figure of the body, and the particular nature of the case, determine the axis round which the solid revolves, while it changes its situation on a fluid's surface; this axis is called, for the sake of distinction, the axis of motion.

The axis of motion, round which the solid revolves, having been determined, and the specific gravity being known, it appears from the preceding observations, that the positions of permanent floating will be obtained, first by finding the

several positions of equilibrium through which the solid may be conceived to pass, while it revolves round the axis of motion; and secondly, by determining in which of those positions the equilibrium is permanent, and in which of them it is momentary and unstable.

In proceeding to investigate the principles which are the objects of the present inquiry, it will be convenient in the first instance to consider the floating body to be some homogeneal solid of regular figure, and uniform shape and dimensions, in respect of the axis of motion throughout. If such a solid is supposed to be cut through by vertical planes in a direction perpendicular to the axis of motion, the sections of these planes with the solid will be areas precisely equal and similar. Let EDHF (Tab. III. fig. 1.) represent the vertical section of such a solid, which passes through the centre of gravity G in a direction perpendicular to the axis of motion. The solid floats on the surface of a fluid IABL; consequently ADHB represents the part immersed under the fluid's surface; O is the centre of gravity of the part immersed, and the line GOC is assumed perpendicular to the horizontal line AB. We are in the next place to suppose that this solid is inclined round the axis of motion from its former position through an angle KGS (fig. 2.);* so that the line KC which was before ver-

* When this inclination takes place, the centre of gravity G, through which the axis of motion passes, is not necessarily fixed, but must evidently in most cases change its place, since the total volume immersed before the inclination is always equal to that which is immersed after the inclination; and from this cause such change of place ensues: but the motion of the axis, and of the point G, is wholly independent of the reasoning in this and the subsequent constructions and investigations; the object of which is to ascertain the angular motion round the said axis, and the several consequences thereof, and is no ways connected with the motion of the axis itself. This note

tical, may be now transferred to the position SL, which is inclined to the vertical line KC at the angle KGS: moreover the line AB, which was before horizontal, is transferred so as to coincide with the line IN, being inclined to its former position in the angle NXP, which is equal to KGS: and consequently the whole space ADHB, becomes transferred so as to coincide with the space IRMN, and the volume immersed under the fluid's surface is WRMNP. If in the line SL, GE is taken equal to GO; it is evident that in consequence of the inclination, the point O, which is the centre of gravity of the space ADHB, will be transferred to the point E, which is the centre of gravity of the equal space IRMN; and the pressure of the fluid would act on the solid in the direction of a vertical line passing through the point E, if the space IRMN was the volume immersed under the fluid's surface; but in consequence of the inclination of the solid through the angle KGS, the volume NXP, which was before above the fluid's surface, will now become immersed under it; and the volume IWX, which was before under the surface, will become elevated above it. It is evident, that on both these accounts, that is, both by the addition of the volume NXP, and the abstraction of the volume IWX, the centre of gravity E of the space IRMN will be transferred towards those parts of the solid which have become more immersed under the fluid in consequence of the inclination.

Suppose the centre of gravity of the volume immersed, WRMP, to be situated at the point Q: through Q draw

is here inserted in preference to adapting the construction so as to express the alteration in the position of the axis, which would only have the effect of embarrassing the construction with useless lines.

QS parallel to GO; through E draw EY perpendicular to SQ; and through G draw *z* GZ perpendicular to SQ. Then, since the point Q is the centre of gravity of the part immersed, the pressure of the fluid will act in the direction of the vertical line QS, with a force equal to the body's weight, and by the principles of mechanics will have precisely the same effect to turn the solid round its axis as if the same force was applied immediately at the point Z, acting in the same direction QS. Since, therefore, the effect of the fluid's pressure acting in the direction of a vertical line which passes through the centre of gravity Q, no way depends on the absolute position of that point, but on the perpendicular distance GZ, between the two vertical lines GO and SQ only, in proceeding to ascertain, by geometrical construction, the several positions which bodies assume on a fluid's surface, and their stability of floating, the determination of the absolute position of the point Q, or centre of gravity of the immersed part, will not be necessary; the perpendicular distance GZ between the two vertical lines which pass through the centres of gravity of the solid, and of the part immersed, being sufficient for obtaining all the results that are required.

The part immersed, before the inclination of the solid took place, is ADHB; when the solid has been inclined through the angle KGS, the part immersed is WRMP, which is the volume IRMN diminished by the space IWX, and augmented by the space NXP. But since the volume immersed under the fluid's surface must always be of the same magnitude while the solid's weight continues unaltered, it follows, that whatever additional space is immersed under the surface in consequence of the inclination, an equal space must be ele-

vated above it; consequently, whatever may be the position of the point of intersection X, the volume I X W must be equal to the volume P X N. Suppose *a* to be the centre of gravity of the space I X W, and let *d* be the centre of gravity of the space N X P; then, the part immersed W R M P, is equal to the space IRMN, diminished by the space IWX, considered as concentered in the point *a*, and increased by the equal space N X P, concentered in the point *d*; consequently the centre of gravity Q of the space W R M P will be at such a distance from E, the centre of gravity of the space IRMN, as corresponds to the alteration occasioned by removing the volume I W X, concentered in the point *a*, to the point *d*. These are the data from which the perpendicular distance G Z, of the two vertical lines K O, S Q, passing through the centres of gravity G and O, is to be obtained in the manner following: through the centres of gravity *a* and *b*, draw the lines *ab*, *dc*, perpendicular to the horizontal line AB; through E draw the indefinite line EY parallel to AB, and in the line EY, take a part ET, so that ET shall be to the line *bc* as the volume IWX, or its equal NXP, is to the whole volume immersed, WRMP or ADHB: through the point T thus found, draw the line F T S parallel to the vertical line G O; the centre of gravity Q, of the immersed part, will be somewhere in the line FS; and because ER is to E G, as the sine of the given angle of inclination is to radius, the line GO = EG being supposed given, the line ER will therefore be known, and being subtracted from the line ET before found, will leave RT or GZ the perpendicular distance between the two vertical lines, which it was required to determine by geometrical construction, and which has been accordingly determined.

The demonstration of this construction is founded on an obvious and elementary principle of mechanics.—It is this.—The common centre of gravity of any system of bodies (considered as heavy points or centres of gravity), being given in position, if one of these bodies should be moved from its place, the corresponding motion of the common centre of gravity, estimated in any given direction, will be to the motion of the aforesaid body, estimated in the same direction, as the weight of the body moved is to the weight of the whole system. To apply this proposition. The volume IRMN (fig. 2.) may be assumed as a system of bodies, of which the common centre of gravity is E. One of the bodies composing this system, namely, the volume I W X, concentered in the point *a*, is transferred in consequence of the inclination of the solid through the angle S G K from the point *a* to the point *d*, in which the equal volume N X P is concentered: this will have the effect of moving the common centre of gravity of the system E. But it is required to find how much the position of this centre E has been changed in the direction EY parallel to A B, which is the given direction stated in the proposition. The motion of the centre of gravity *a*, from *a* to *d*, estimated in the given horizontal direction, is *bc*: then, according to the mechanical proposition, as the volume W R M P or A D H B is to the volume I W X or N X P, so is the line *bc* to ET, the corresponding motion of the centre of gravity E estimated in the given horizontal direction; consequently if a line FTS is drawn through the point T parallel to the vertical line GO, the centre of gravity of the immersed part Q must be situated somewhere in the line FTS: subtracting from ET the line E R (which is the sine of the given

angle of inclination EGO when EO is the radius), there will remain the line RT or GZ, which is therefore the distance between the vertical lines GO, SZT, passing through the centres of gravity G and Q, as determined by the construction.

Let the whole volume of the immersed part of the solid be denoted by the letter V; suppose the space NXP, or volume immersed in consequence of the inclination, to be A; make GO = d; and the sine of the angle of inclination KGS (to radius 1) = s; also make $bc = b$. Then since by the proposition; as b : ET : : V : A, it appears that ET = $\frac{b \times A}{V}$;

And since as ER : EG = GO : : so is s : 1, we obtain ER = ds;

Wherefore RT = ET — ER = $\frac{bA}{V} - ds$ = GZ.

This result is founded on a supposition that the figure of the floating solid is uniform in respect of the axis of motion; if the solid should be of an irregular form, the construction and demonstration will be precisely the same as in the preceding case, the following particulars being attended to; the volume, or space immersed in consequence of the inclination, will no longer be represented by the area NXP, but must be obtained by a calculation founded on the shape and dimensions of the said volume; moreover the centres of gravity of the volumes PXN, IXW, will not now correspond with the centres of gravity of the areas PXN, IXW, and must therefore be obtained from the known rules, or from methods of approximation by which the position of the centre of gravity is determined in solid bodies.

The angle of inclination KGS is given by the supposition, and the solid contents of the equal volumes denoted by IXW, NXP, with the distance bc of the centres of gravity a and d,

estimated in the direction of the horizontal line AB, having been determined, let the volume NXP be put = A; and $bc = b$; the other quantities signifying as before; the perpendicular distance $GZ = \frac{bA}{V} - ds$, will be known. It is to be observed, that this proposition in general is equally applicable to heterogeneal bodies as to those which are homogeneal.

By this proposition the stability of vessels, and other bodies floating on a fluid's surface, at any angle of inclination, from a given position of equilibrium, is obtained. For the measure of the stability is precisely a force equal to the fluid's pressure; that is, equal to the vessel's weight,* applied perpendicularly at the distance GZ from the axis of motion, to incline the solid round that axis.

From the same proposition, the different positions assumed by bodies which float freely on a fluid's surface, may be ascertained; in some cases most easily by geometrical construction; in others, by analytical investigation. It has been already observed, that to ascertain the various positions in which a body will float permanently on the surface of a fluid, it is necessary first, to have given the ratio of the specific gravities, in order to fix the proportion of the part immersed to the whole; and secondly, the several positions are to be ascertained in which the solid may rest on the surface of a fluid, so that the centres of gravity of the solid and of the part immersed may be in the same vertical line. The general expression for the line RT (fig. 2.) or GZ, is $GZ = \frac{bA}{V} - ds$; by putting this quantity $\frac{bA}{V} - ds = 0$, an equation arises, from which one or more values of s will be obtained = the sine of the angle through which the solid has been inclined from a position of

* The weight of a vessel implies the weight of the ship and lading.

equilibrium, when the line GZ = o; that is, when the two centres of gravity, G and Q, are again situated in the same vertical line; or in other words, when the solid is again in a position of equilibrium. By this method of proceeding, the several positions of equilibrium may be determined; it only, therefore, remains to discover in which of these positions the equilibrium is permanent, and in which of them it is momentary and unstable. This circumstance will depend on the species of equilibrium in which the solid is originally placed previously to the inclination, which, for the sake of more clearly stating the principles of stability, may be supposed known, although the rules for ascertaining this point have not yet been considered, but will appear in the pages which next follow. Assuming then the species of equilibrium, in which a solid is originally placed on the surface of a fluid, to be known, let that equilibrium be supposed permanent, or the equilibrium of stability; and let the solid be conceived to be inclined round the axis of motion, through a given angle A, till it becomes situated again in a position of equilibrium; in which case the centres of gravity of the solid, and of the part immersed, will again be in the same vertical line. Since during this inclination, the fluid's pressure acts with a force proportional to the line RT or GZ, (fig. 2.) to diminish the angular distance from the original position of equilibrium, it follows that the same force must act on the solid, so as to augment the inclination, or angular distance from the second position of equilibrium, in which the solid is situated after it has revolved through the entire angle A, or any part thereof, from its original situation; from which observations it is evident, that the second position of equilibrium must be that of instability:* and by the same

* It appears from the observations in page 49, that whenever a solid floats in a posi-

mode of argument it is shewn, that if the original position of equilibrium be that of instability, when the solid by revolving on its axis has become situated in a second position of equilibrium, it will float permanently, that is with stability, in that second position. And in general, when a floating solid revolves round a given horizontal axis, and passes through several positions of equilibrium, those of stability and instability are alternate, no position of either species following immediately a position of the same species. In order, therefore, to find what position a solid will assume after it has overset from any situation of unstable equilibrium, it is only necessary to ascertain the angle of inclination from the given situation through which the solid must revolve on the axis of motion, so that the distance GZ (fig. 2.) between the two vertical lines which pass through the centre of gravity of the solid and the centre of gravity of the part immersed may become evanescent. It is necessary in the next place, to determine whether any position of equilibrium originally given is that of stability or instability. This point will be ascertained by having recourse to the general value which has been investigated, for expressing the distance between the two vertical lines GO, ST (fig. 2.); or $GZ = \frac{A\,b}{V} - ds$. In the line ER take any point t, and through t draw qtz parallel to GO. As long as $\frac{b\,A}{V} = ET$ is greater than $ds = ER$, the point Z, and the line of support QZ, will be between the axis and those parts of the solid which are immersed by the inclination,

tion of permanent equilibrium, and is deflected from that position through a small angle, the force of the fluid's pressure causes the solid to revolve round its axis in a direction contrary to the inclination; and if the equilibrium is unstable, the same force acts to increase the said inclination; this latter case corresponds to that of the equilibrium in which the solid is situated after it has revolved through the angle A.

the consequence of which is an equilibrium of stability; and whenever $\frac{bA}{V} = ET$ is less than $ds = ER$, the point q, and the line of support qz, will be on the contrary side of the axis, causing an equilibrium of instability to take place.* The equation, therefore, $GZ = \frac{bA}{V} - ds$, applied to any particular case, will always decide whether the equilibrium in which a solid is placed on the surface of a fluid is stable and permanent, or whether it is only momentary and unstable, provided the value of s, or the sine of the angle of inclination from the given position of equilibrium, be assumed evanescent; since the solid either continues to float permanently, or will overset, according to circumstances which take place while it is inclined from its position of equilibrium through the smallest angle. The application of the condition just mentioned will cause the general expression to assume a form suited to this particular case, which is in the next place to be attended to.

Referring to (fig. 2.), ADHB represents a vertical section of a floating body, passing in a direction perpendicular to the axis of motion; suppose another section to be drawn parallel to the former, and extremely near to it; these two planes will comprehend between them a small portion of the solid; and since according to the conditions of the case, the angle of inclination KGS, or NXB, is evanescent, the sine of this angle (which has been denoted by the letter *s*) will also become evanescent; and since the space or volume immersed in consequence of the inclination, that is NXP, is equal to the volume elevated above the surface IXW, and the angles NXP, IXW, are vertical; the point of intersection of the lines IN and AB, that is, the point X will bisect the line AB, and the

* Page 49, and page 50.

points P, B, and N, will coincide; on which account the evanescent area NXP will be $= \frac{\overline{XB}^2 \times s}{2} = \frac{\overline{AB}^2 \times s}{8}$; and if z is put to represent a line drawn through the middle of the solid, on a level with the fluid's surface, and parallel to the longer axis, the evanescent portion of the solid intercepted between the two adjacent planes, will be $\frac{\overline{AB}^2 \times s}{8} \times \dot{z}$: the perpendicular distance of the centre of gravity of this evanescent solid from the point X, is $\frac{1}{3}$ AB. But it is required in the present instance to assign the distance from the horizontal line passing through the point X, of the centre of gravity of the entire volume immersed by the inclination, that is, the common centre of gravity of all the evanescent solids $\frac{\overline{AB}^2 \times s\dot{z}}{8}$ corresponding to the entire length z. This distance may be obtained from the known rule of mechanics, which is, by multiplying each evanescent solid, considered as concentered in its centre of gravity, into the distance of that centre from the given line, and dividing the sum of the products by the sum of the solids; the result will be, the distance of the common centre of gravity from the horizontal line passing through the point X parallel to the axis; and since the evanescent solid corresponding to the small lineal increment $\dot{z}$ is $\frac{\overline{AB}^2 \times s \times \dot{z}}{8}$, and the distance of its centre of gravity from the point $X = \frac{2XB}{3}$ or $\frac{AB}{3}$, the product arising from multiplying the solid into the distance of its centre of gravity, from the given horizontal line passing through X, will be $\frac{\overline{AB}^3 \times s \times \dot{z}}{24}$; and the sum of all those products corresponding

to the whole length of the line z will be $\frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{24}$; and therefore the distance of the common centre of gravity of the volume immersed in consequence of the inclination from the horizontal line passing through the point X, is $\frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{24A}$; in like manner the distance of the common centre of gravity of the volume, elevated above the surface by the inclination of the given plane, appears to be $\frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{24A}$; and consequently the distance between the two centres of gravity measured on the horizontal line, or bc (fig. 2.) $= \frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{12A}$: this value being substituted for b in the equation $GZ = \frac{bA}{V} - ds$, we obtain the following result, *i. e.* $GZ = \frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{12V} - ds$, which is a general expression for ascertaining whether a solid, when placed on the surface of a fluid in a given position, will float permanently, or overset, the sine of the angle of inclination or s being assumed evanescent; for, when $\frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{12V}$ is greater than ds, the line of support QZ (fig. 2.) will be situated between the axis of motion, and the parts of the solid which are immersed by the inclination, in which case the solid will float permanently; and when $\frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{12V}$ is less than ds, the line of support passing through the point z will be on the contrary side of the axis, and according to the preceding determination (page 64) the solid will in this case overset.

Since, when the fluent of $\frac{\overline{AB}^3 s \dot{z}}{12V}$ (fig. 2.) is greater than ds,

the solid floats permanently; and when ds is greater than $\frac{\text{fluent of } \overline{AB}^3 \, s\dot{z}}{12\,V}$, the equilibrium is that of instability; it follows that whenever $\frac{\text{fluent of } \overline{AB}^3 \, s\dot{z}}{12\,V} = ds$, by resolving the equation $\frac{\text{fluent of } \overline{AB}^3 \, \dot{z}}{12\,V} = d$, one or more limits are obtained (depending on the dimensions and specific gravity of the solid), separating the cases in which the solid floats with stability from those in which the equilibrium is momentary and unstable. The limits here obtained evidently correspond to that species of equilibrium which has been denominated insensible, or the equilibrium of indifference.

When the floating body is of uniform figure and dimensions, respecting the axis of motion, the expression here given for determining the stability or instability of floating will not involve any fluxional quantities, for in this case all the vertical sections which pass through the solid in a direction perpendicular to the axis are equal, and consequently the portions of those sections immersed under the fluid's surface are also equal; if, therefore, the area of any one of these sections immersed under the fluid's surface be denoted by the letter D, the solid contents or volume immersed, corresponding to the length of the line z, will be Dz; wherefore, in the preceding expression $GZ = \frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{12\,V} - ds$, we have by substitution $V = Dz$, and since AB is a constant or invariable quantity by the supposition, $\frac{\text{fluent of } AB^3 \, s\dot{z}}{12\,Dz} = \frac{AB^3 \, sz}{12\,Dz} = \frac{AB^3 \times s}{12\,D}$: finally, therefore, in the case under consideration, we obtain $GZ = \frac{\overline{AB}^3 \times s}{12\,D} - ds$.

In the subsequent pages, cases occur in which each of the preceding expressions are employed, not only to ascertain the laws of permanent and unstable equilibrium, but in developing other properties relating to the subject.

EFCD (fig. 3.) represents a vertical section of an oblong solid or parallelopiped, placed on the surface of a fluid IABK, with one of the flat surfaces upward, or the line CE or FD vertical: this solid is moveable round an horizontal axis, which passes through the centre of gravity G, perpendicular to the plane ECDF. Let it be required to determine the limits, depending on the dimensions and specific gravity of the solid, which separate the cases in which the solid will float permanently, from those in which it will overset; through the centre of gravity G draw the line SGL parallel to CE or DF: let the height of the solid $CE = c$; let the base $CD = a$; also let the specific gravity of the solid be to that of the fluid on which it floats in the proportion of n to 1, or as SN to SL; so that when it is placed on the fluid with the line SL vertical, it may sink to the depth SN; let O be the centre of gravity of the part immersed: suppose the solid to be placed on the surface of the fluid with the line SL vertical; then, since SN is the depth to which the solid sinks in the fluid, and SN is to SL as n to 1, it follows that $SN = nc$; and consequently $GO = \frac{c}{2} - \frac{nc}{2}$; the area immersed $ABCD = acn$; wherefore, to ascertain the perpendicular distance between the two verticals which pass through the centres of gravity of the solid and of the part immersed, when the solid is inclined through a very small angle, of which the sine is $= s$ to radius 1, re-

ferring to the general expression * $GZ = \frac{\overline{AB}^3 \times s}{12D} - ds$, we obtain the following values $AB = a$, $D = acn$, $d = \frac{c - nc}{2}$, and therefore $GZ = \frac{a^3 s}{12 acn} - \frac{s \times \overline{c - nc}}{2}$: by making the distance $GZ = o$, we obtain an equation expressing the relation of the dimensions and specific gravity of the solid, when the equilibrium becomes insensible, that is, when the centres of gravity of the solid and of the part immersed remain in the same vertical line, however the value of s or the sine of the inclination from the upright position may be altered, provided it is always very small; making, therefore, $\frac{a^3 s}{12 acn} = \frac{s \times \overline{c - cn}}{2}$, we have $6c^2 n^2 - 6c^2 n = -a^2$ and $n^2 - n = -\frac{a^2 s}{6c^2}$, which gives $n = \frac{1}{2} + \sqrt{\frac{1}{4} - \frac{a^2}{6c^2}}$, or $n = \frac{1}{2} - \sqrt{\frac{1}{4} - \frac{a^2}{6c^2}}$: from whence the following inference is obtained, *i. e.* in all cases whenever $\frac{a^2}{6c^2}$ is less than $\frac{1}{4}$, that is, whenever the height of the solid c bears to the base a a greater proportion than that of $\sqrt{2}$ to $\sqrt{3}$, two values may be assigned to the specific gravity of the solid, each of which will cause it to float in the insensible equilibrium: thus, suppose the height c to be to the base a in the proportion of equality: to ascertain the two limiting specific gravities, by referring to the preceding solution, and making $c = a$, we obtain $n = \frac{1}{2} - \sqrt{\frac{1}{4} - \frac{1}{6}}$, or $n = \frac{1}{2} + \sqrt{\frac{1}{4} - \frac{1}{6}}$, that is $n = \frac{1}{2} - .28868 = .21132$,

or $n = \frac{1}{2} + .28868 = .78868$.

* When the angle KGS in fig. 2. is evanescent, the line GZ vanishes: this being the case represented by fig. 3, the point Z coincides with the point G.

From the equation $GZ = \frac{a^3 s}{12 acn} - \frac{s \times \overline{c - cn}}{2}$ it is inferred, that when the specific gravity of the solid is of very small value in respect to that of the fluid, because $\frac{a^3 s}{12 acn}$ must in this case be necessarily greater than $\frac{s \times \overline{c - cn}}{2}$ the solid will float permanently with the line SL vertical, that is, with the flat surface EF parallel to the horizon. Secondly, the specific gravity .21183 causing the solid to float in the insensible equilibrium, is the limit at which the solid ceases to float with stability; if therefore the specific gravity is increased beyond .21183, and the solid is placed on the fluid with the flat surface upward, the equilibrium thus formed will be that of instability, from which the solid will be deflected into some other position in which the equilibrium is permanent. While the specific gravity is augmented from .211 to .788, the instability increases at first, but admits of a maximum, which is found by putting the least increment of the quantity $\frac{a^3 s}{12 acn} - \frac{s \times \overline{c - cn}}{2} = 0$, considering n as a variable quantity, and making $a = c$; in which case n appears to be equal to $\frac{1}{\sqrt{6}}$. If the value of the specific gravity is increased beyond $\frac{1}{\sqrt{6}}$, the instability becomes less, and at last vanishes when the specific gravity is at its second limit = .78868: whatever value is given to the specific gravity between .78868 and 1, the solid will float permanently with the line SL vertical, or with its flat surface horizontal.

These cases arise from assuming the height of the parallelopiped SL, in a greater proportion to its base CD than that of

$\sqrt{2}$ to $\sqrt{3}$; and from the same solution it appears, that if the height bears a less proportion to the base than that of $\sqrt{2}$ to $\sqrt{3}$, no value can be given to the specific gravity, which will cause the stability to vanish, because the quantity $\sqrt{\frac{1}{4} - \frac{a^2}{6c^2}}$ becomes impossible; in which case the solid placed with the surface EF horizontal, must in all cases continue to float permanently in that position, whatever may be the specific gravity, always supposed to be less than that of the fluid.

(Fig. 4.) Similar determinations may be obtained from the same theorem respecting the equilibrium of the solid, when placed on a fluid with a plane angle upward, that is, with the diagonal line EGC vertical. Let EDCF represent a vertical section of a square parallelopiped floating on the surface of a fluid IABK: making the side $DC = a$, the line $GC = \frac{a}{\sqrt{2}}$, suppose that the specific gravity of the solid is to the specific gravity of the fluid as n to 1, and that the solid sinks in the fluid to the depth HC; let G be the centre of gravity of the solid, and O the centre of gravity of the part immersed; then the area ABC is to the area DEFC as n to 1; wherefore the space $ABC = \overline{HB}^2 = a^2 n$, and $HB = HC = a \times \sqrt{n}$; $AB = 2a\sqrt{n}$; $OC = \frac{2a\sqrt{n}}{3}$ and $GO = \frac{a}{\sqrt{2}} - \frac{2a\sqrt{n}}{3} = \frac{a \times 3 - \sqrt{8 \times n}}{\sqrt{2} \times 3}$.

Referring to the quantity expressing the perpendicular distance between the two vertical lines passing through the centre of gravity of the solid, and the centre of gravity of the part immersed, when the angles of inclination from the

position of equilibrium, are very small, that is, $GZ = \frac{\overline{AB}^3 \times s}{12D} - ds$, and applying this equation to the case under consideration, we obtain the following values; $AB^3 = 8a^3 \times n^{\frac{3}{2}}$; $D = a^2 n$; $d = \frac{a \times \overline{3 - \sqrt{8n}}}{\sqrt{2} \times 3}$: making therefore $\frac{AB^3 s}{12D} = ds$, in order to obtain the limit, separating the cases of stability and instability of floating; or, which is the same thing, making $\frac{8a^3 n^{\frac{3}{2}}}{12a^2 n} = \frac{a \times \overline{3 - \sqrt{8n}}}{\sqrt{2} \times 3}$, the following equation arises, $\frac{2\sqrt{n}}{3} = \frac{3 - \sqrt{8n}}{\sqrt{2} \times 3}$, or $n = \frac{9}{32} = .28123 =$ the specific gravity, which will cause the solid to float in the insensible equilibrium, and is therefore the limit separating the specific gravities which cause the solid to float with stability from those which produce the equilibrium of instability. It is collected from the general equation $GZ = \frac{\overline{AB}^3 s}{12D} - ds$, or $GZ = \frac{8a^3 n^{\frac{3}{2}} s}{12a^2 n} - \frac{a \times \overline{3 - \sqrt{8n}}}{\sqrt{2} \times 3}$; that when the specific gravity (n) is evanescent or very small, the solid will overset when placed on the fluid with an angle upward, because in this case the quantity $\frac{8a n^{\frac{3}{2}} s}{12a^2 n}$ must necessarily be less than $\frac{a \times \overline{3 - \sqrt{8n}}}{\sqrt{2} \times 3}$, or ds. When the specific gravity of the solid is to that of the fluid in the proportion of 9 to 32, the solid floats in the insensible equilibrium; if therefore the specific gravity of the solid should be to that of the fluid in a less proportion than that of 9 to 32, the solid will overset; but if the specific gravity of the solid exceeds that limit when placed on the fluid with the angle upward, or diagonal line EC vertical, it will float permanently in that position.

Respecting this determination it seems remarkable, that there should be only one value of specific gravity, as a limit between the stability and instability of floating; whereas there were two specific gravities, each of which was a limit in the case when the solid was placed on the fluid with a flat surface upward. This difficulty admits of very satisfactory explanation; when the flat surface is placed upward, the conditions on which the solution is founded are not at all altered, to whatever depth the solid may sink: but in the present case, when the solid is placed on the fluid with a plane angle upward, the conditions on which the solution has been investigated imply, that as the specific gravity is increased, the section of the solid formed by the fluid's surface shall continually increase also; and on that ground the result justly gives one limit only between the stability and instability of floating; but since in reality the section of the solid by the fluid's surface increases only until the specific gravity becomes one half of that of the fluid, and afterwards decreases, it is evident, that if there should be another limit corresponding to the case when the specific gravity is greater than one-half, it must be discovered by a separate investigation. Let, therefore, the square parallelopiped EDCF (fig. 5.) of which the specific gravity is greater than $\frac{1}{2}$, that of the fluid being 1, be placed on the fluid with the diagonal line EC vertical: IABK represents the surface of the fluid, and HC the depth to which the solid sinks; G is the centre of gravity of the solid, and O the centre of gravity of the part immersed. If one of the sides DE is made $= a$, and the specific gravity put $= n$, then the area ABDCFA $= a^2 n$; and the area EAB $= a^2 - a^2 n = \overline{EH}^2$; wherefore $EH = a \times \sqrt{1 - n} = AH$; $AB = 2a \times$

$\sqrt{1-n}$; and $GH = a \times \frac{1}{\sqrt{2}} - \sqrt{1-n}$: let P represent the centre of gravity of the area AEB; then by the properties of the centre of gravity the following equation arises:
GH × area EDCF = area ABDCFA × OH — area AEB × HP,
that is

$a^3 \times \frac{1}{\sqrt{2}} - \sqrt{1-n} = a^2 n \times OH - a^3 \times \frac{1-n^{\frac{3}{2}}}{3}$; and consequently $HO = \frac{a \times 3 - \sqrt{18} \times \sqrt{1-n} + a \times \sqrt{2} \times 1-n^{\frac{3}{2}}}{\sqrt{18} \times n}$; from which quantity taking away the line $HG = a \times \frac{1}{\sqrt{2}} - \sqrt{1-n} = \frac{3n - \sqrt{18n^2} \times \sqrt{1-n}}{\sqrt{18} \times n}$, there will remain the line $GO = \frac{a \times 3 - 3n - \sqrt{18} \times 1-n^{\frac{3}{2}} + \sqrt{2} \times 1-n^{\frac{3}{2}}}{\sqrt{18} \times n}$.

Referring to the general expression, namely $\frac{AB^3 s}{12D} - ds$, we obtain in the present case $AB^3 = 8a^3 \times 1-n^{\frac{3}{2}}$, $D = a^2 n$, $GO = d = \frac{a \times 3 - 3n - \sqrt{18} \times 1-n^{\frac{3}{2}} + \sqrt{2} \times 1-n^{\frac{3}{2}}}{\sqrt{18} \times n}$; wherefore $\frac{AB^3}{12D} - d = \frac{8a^3 \times 1-n^{\frac{3}{2}}}{12a^2 n} - \frac{a \times 3 - 3n - \sqrt{18} \times 1-n^{\frac{3}{2}} + \sqrt{2} \times 1-n^{\frac{3}{2}}}{\sqrt{18} \times n}$; which quantity being put equal to o, in order to obtain the limit, and the whole being multiplied by $\frac{3n \times \sqrt{2}}{1-n \times a}$, will give $2\sqrt{2} \times \sqrt{1-n} = 3 - 3\sqrt{2} \times \sqrt{1-n} + \sqrt{2} \times \sqrt{1-n}$, or $\sqrt{1-n} = \frac{3}{4\sqrt{2}}$; wherefore $1 - n = \frac{9}{32}$ or $n = \frac{23}{32}$, the limit required.

By the preceding determinations of the four limiting values of

the specific gravity, *i.e.* $\frac{1}{2} - \sqrt{\frac{1}{4} - \frac{1}{6}}$, $\frac{9}{32}$, $\frac{23}{32}$, & $\frac{1}{2} + \sqrt{\frac{1}{4} - \frac{1}{6}}$:
or .211, .281, .718, & .789, we find that if the specific gravity is less than .211, the square parallelopiped, when placed on the surface of the fluid with a flat surface upward and horizontal, floats permanently in that position, but oversets if the specific gravity is greater than .211, and less than .789. We observe also, that when the solid is placed on the fluid with an angle upward, if the specific gravity is less than .281 it oversets; if greater than .281 and less than .718, the solid floats permanently with the angle upward; but if the specific gravity exceeds .718, the solid oversets when placed on the fluid with an angle upward.

It is therefore evident at what depth of floating, depending on the specific gravity, the solid when placed on the fluid in the positions which have been described, begins or ceases to float with stability. But a material inquiry remains to be considered, which is, to ascertain in what position a square parallelopiped will dispose itself, in respect to the fluid's surface, when the specific gravity is of any intermediate values between the limits which have been determined. To resolve this question the preceding results are evidently inadequate, since from these we only know in what cases, depending on the values of the specific gravity, the solid when placed on the fluid either with a flat surface or an angle upward will float permanently; and in what cases it will overset. Suppose the latter event to take place, and that the solid, having been placed on the fluid in a position of unstable equilibrium, oversets or changes its position by revolving on its axis. To ascertain what position the solid so circumstanced will assume, in which

it will continue permanently to float, we must have recourse to the theorem for expressing the perpendicular distance between the two verticals, which pass through the centres of gravity of the solid and of the part immersed. For by putting this value = o, the resolution of an equation thence arising, will give the sine of the inclination from the position of equilibrium at which these two vertical lines coincide; that is, when the centres of gravity of the solid and of the part immersed are again in the same vertical line: in this case the solid will be situated in a position of equilibrium, which, according to the observations in page 63, must be an equilibrium of stability.

Let EFDC (fig. 6.) represent the vertical section passing through the centre of gravity G of an oblong solid or parallelopiped, the longer axis of which passes through the centre of gravity G in a direction perpendicular to the plane EFCD; LGS is drawn through G parallel to CE or DF; this solid is placed on the surface of a fluid IABK, with the line SGL vertical; and the specific gravity of the solid is such as causes it to sink to the depth under the fluid's surface SN.

The volume immersed under the fluid's surface is the space ACDB, of which the centre of gravity is O; and since the points G and O are situated in the same vertical line, the solid will be in a position of equilibrium, which, according to the present supposition, is assumed to be the equilibrium of instability; the solid will therefore spontaneously overset whenever external support is removed, and will change its position by revolving round an horizontal axis which passes through the centre of gravity in a direction perpendicular to the plane CDFE.

It is required to ascertain through what angle WGS, the solid will be inclined round its axis, when the centres of gravity of the solid and of the part immersed are again in the same vertical line. As in the former cases, this problem will be solved, by referring to the general expression for the distance between the two vertical lines which pass through the centres of gravity of the solid and of the part immersed.

Suppose then the solid to be inclined from its former position of equilibrium in an angle WGS, so as to become transferred from the position ECDF into the position YWHV; the part immersed will now be ZHVR; the line AB will also be transferred to PQ, and the space QXR, which was before above the fluid's surface, will now be immersed under it; and the space PXZ, which was before under the surface, will now be above it. Bisect the lines PZ, QR, in *m* and *n*, and join *m*X, *n*X; and take X*a* = $\frac{2}{3}$ of X*m* and X*d* = $\frac{2}{3}$ of X*n*; so shall *a* and *d* be the centres of gravity of the triangles PXZ, QXR, respectively; draw the lines *ab*, *cd*, perpendicular to the horizontal line AB. Referring to the quantity expressing the distance between the vertical lines which pass through the centres of gravity of the solid and of the part immersed, namely, $\frac{bA}{V} - ds$, there will be applicable to the present case, the space QXR = A; the space ZHVR or ACDB = V; *bc* = *b*; OG = *d*; the sine of the angle of inclination or WGO = *s*: let *t* be the tangent of the same angle to radius = 1; then, since the triangles ZXP, QXR, are similar, and the areas are equal by the supposition, the sides of the two triangles will be respectively equal; that is, QX will be equal to XP; ZP equal to QR; and ZX to XR. Let the height of the solid SL = *c*,

and the specific gravity $= n$ when that of the fluid is equal to 1, also make VW or XQ $= a$; then QR $= at$, and Q$n = \frac{at}{2}$; X$n = \sqrt{a^2 + \frac{t^2 a^2}{4}}$, or X$n = \frac{a}{2} \times \sqrt{4 + t^2}$.

To find the sine of the angle nXR, make the following proportion. As Rn or Qn $\left(\frac{ta}{2}\right)$: Xn $\left(\frac{a}{2} \times \sqrt{4 + t^2}\right)$: : sine nXR : sine XRn : wherefore sine nXR $= \frac{\text{sine } n\text{RX} \times t}{\sqrt{4 + t^2}}$; or because sine nRX $= \frac{1}{\sqrt{1 + t^2}}$, sine nXR $= \frac{t}{\sqrt{4 + t^2} \times \sqrt{1 + t^2}}$; cos. nXR $= \frac{2 + t^2}{\sqrt{4 + t^2} \times \sqrt{1 + t^2}}$: and since X$d = \frac{2}{3} \times$ X$n =$ $\frac{a \times \sqrt{4 + t^2}}{3}$, it follows that X$c = \frac{a \times \sqrt{4 + t^2} \times \sqrt{2 + t^2}}{3n \sqrt{4 + t^2} \times \sqrt{1 + t^2}} = \frac{a}{3} \times$ $\frac{2 + t^2}{\sqrt{1 + t^2}}$; and since the triangles XPZ, XQR, as also the triangles ZXm, RXn, are similar and equal, the line X$b =$ Xc; and consequently $bc = 2$X$c = \frac{2a \times \sqrt{2 + t^2}}{3 \times \sqrt{1 + t^2}}$; which quantity $=$ b in the general value $\frac{b\text{A}}{\text{V}} - ds$. And since the specific gravity of the solid is $= n$, the height SL $= c$, and the base CD $= 2a$, the immersed part or ACDB $= 2acn$, which in the general expression is denoted by V; and the volume QXR $=$ $\frac{a^2 t}{2}$ is denoted by the letter A in such general value.

Substituting, therefore, in the expression $\frac{b\text{A}}{\text{V}} - ds$, $\frac{2a \times \sqrt{2 + t^2}}{3 \times \sqrt{1 + t^2}}$ for b; $\frac{a^2 t}{2}$ for A; and $2acn$ for V; the distance between the vertical lines passing through the centres of gravity of the solid and the centre of gravity of the part immersed, appears

to be $\frac{2a \times \overline{2+t^2}}{3 \times \sqrt{1+t^2}} \times \frac{a^2 t}{2 \times 2acn} - ds$, or $\frac{a^2 t \times \overline{2+t^2}}{6cn \times \sqrt{1+t^2}} - ds$; or since $d = \frac{c-cn}{2}$, the said distance $= \frac{a^2 t \times \overline{2+t^2}}{6cn \times \sqrt{1+t^2}} - \frac{\overline{c-cn} \times s}{2}$; or by substituting for t^2 its value $\frac{s^2}{1-s^2}$, the distance $= \frac{a^2 s \times \overline{2-s^2}}{6cn \times \overline{1-s^2}} - \frac{\overline{c-cn} \times s}{2}$; in which expression a denotes half the breadth PQ; but as it may be more convenient to represent the whole breadth AB or PQ by the letter a, the expression will in this case be $= \frac{a^2 s \times \overline{2-s^2}}{24cn \times 1-s^2} - \frac{\overline{c-cn} \times s}{2}$; which quantity being put $= 0$, we obtain $s^2 = \frac{2a^2 - 12c^2 n + 12c^2 n^2}{12c^2 n^2 - 12c^2 n + a^2}$, or $s^2 = \frac{12c^2 n - 12c^2 n^2 - 2a^2}{12c^2 n - 12c^2 n^2 - a^2}$. From this equation the angle of inclination from the original position of equilibrium may be found, from having given the specific gravity; or conversely, the specific gravity may be found from having given the angle of inclination through which the solid must revolve, so as to be situated in a second position of equilibrium. As the instances given to illustrate the propositions already investigated have been adapted to the case of a square parallelopiped, the present result may be exemplified on the same supposition. Assuming then the height of the solid to be equal to the base, a will become $= c$ in the preceding expression, and consequently $s^2 = \frac{12n - 12n^2 - 2}{12n - 12n^2 - 1}$.

We have seen in a foregoing proposition, that if the specific gravity of this square solid should be greater than .211 so as not to exceed .789, the solid placed on the fluid with a flat surface upward, would be situated in an equilibrium of instability, and consequently must change its position by revolving on

its axis till it settles in some other position wherein the equilibrium is permanent.

From the present proposition we shall be enabled to ascertain what that position is. Thus, let the specific gravity $n =$ .24, which is between the limits .211 and .789; and will consequently place the solid with a flat surface upward and horizontal, in an equilibrium of instability. By referring to the equation $s^2 = \frac{12n - 12n^2 - 2}{12n - 12n^2 - 1}$, and substituting .24 for n, we find that $s^2 = \frac{12n - 12n^2 - 2}{12n - 12n^2 - 1} = \frac{.1888}{1.1888}$; and $s =$ the sine of 23° 29′. From this calculation it appears, that the solid after having overset from its position of unstable equilibrium, with the flat surface upward and horizontal, and having revolved through an angle of 23° 29′, will settle in a position of permanent equilibrium at that angular distance from its original situation; for by the solution, when the solid has revolved through that angle, the centres of gravity of the solid and of the part immersed are again situated in the same vertical line, and consequently the solid is then situated in a position of equilibrium, which must be the equilibrium of stability, because the original position from which the solid inclined, was that of instability; and it has been observed previously, that when a solid changes its position by revolving on an axis on the surface of a fluid, any position of equilibrium is always succeeded by a position of equilibrium which is of a contrary description.

If the angle of inclination from the upright position with a flat surface horizontal should be given, the specific gravity of the solid may be inferred from the preceding equation, which will cause the solid to float in a position of equilibrium at

that given angle of inclination; for by solving the equation $s^2 = \frac{12n - 12n^2 - 2}{12n - 12n^2 - 1}$ we obtain $n = \frac{1}{2} \pm \sqrt{\frac{1 - 2s^2}{12 - 12s^2}}$. Thus, if it should be required to ascertain the specific gravity which will cause the solid to float *in equilibrio* at the angular distance of 23° 29′ from the upright, we have $\sqrt{\frac{1 - 2s^2}{12 - 12s^2}} =$ 0.26000, and the specific gravity required, that is, $n = .5 + .26 = .76$, or $n = .5 - .26 = .24$. Thus we find from this calculation that there are two specific gravities which will cause the solid to float in a position of equilibrium at the same angular distance 23° 29′ from the original situation with a flat surface horizontal; a conclusion which it is easy to verify by substituting .76 for n in the equation $\frac{12n - 12n^2 - 2}{12n - 12n^2 - 1} = s^2$: the result is that $s^2 = \frac{.1888}{1.1888}$, the same as in the former instance, when n was assumed $= .24$.

In the application of analytical investigation to the solution of problems, it is always necessary to keep distinctly in view the conditions on which the investigation has been founded; for however correct the solution may otherwise have been, any inadvertence in this respect will unavoidably lead to error and inconsistency. The investigation by which the floating position of the solid is determined after it has changed its position from an equilibrium of instability, when one of the flat surfaces was parallel to the horizon, has proceeded on a supposition that the surface of the fluid intersects the parallel surfaces YH, WV, (fig. 6.) in the points R and Z; but if the two surfaces intersected by the fluid should be the inclined sides HV, VW, or in other words, if the point of intersection Z should be situated between H and V, neither the geometrical construction

nor the analytical investigation depending on it, can be applied, so as to ascertain the required position of equilibrium, a solution altogether different being required to determine the position in which a solid under these conditions will float permanently. It is, however, certain, that as long as the point of intersection Z is not lower than the point of the base H, the preceding solution will be applicable: it will be therefore material to find both the angle of inclination from the original position of unstable equilibrium, and the specific gravity of the solid when it floats permanently, with this condition annexed, *i. e.* that the surface of the fluid shall pass through one of the extremities of the base: the result of this solution will form a limiting value both of the angle of inclination and of the specific gravity, beyond which the preceding investigation not being applicable, another solution is required.

Let AECD (Tab. IV. fig. 7.) represent a vertical section of the square parallelopiped which rests permanently on the surface of the fluid IKDH, passing through the extremity of the base D. It is required to find the angle of inclination KDC from a position of equilibrium with a flat surface horizontal, and the specific gravity of the solid, when it floats in a state of equilibrium. Let the tangent of the required angle KDC be to radius as t to 1, and put $CD = a$; let the specific gravity of the solid be to that of the fluid as n to 1. Then $KC = at$, and the area $KCD = \frac{a^2 t}{2}$: and because as the area KCD is to the area AECD, so is n to 1, it follows that $n = \frac{t}{2}$; and since by the preceding investigation* $s^2 = \frac{12n - 12n^2 - 2}{12n - 12n^2 - 1}$, where s represents the sine of the angle of inclination from the

* Page 79.

upright position, which is the angle KDC in the present case; substituting for n its value $\frac{1}{2}$, the equation will now become $s^2 = \frac{6t - 3t^2 - 2}{6t - 3t^2 - 1}$, or because $s^2 = \frac{t^2}{1 + t^2}$, $\frac{t^2}{1 + t^2} = \frac{6t - 3t^2 - 2}{6t - 3t^2 - 1}$, or $6t^3 - 3t^4 - t^2 = 6t - 3t^2 - 2 + 6t^3 - 3t^4 - 2t^2$, or $4t^2 = 6t - 2$; which equation being resolved, gives $t = \frac{3}{4} \pm \frac{1}{4}$, that is, $t = \frac{1}{2}$ or $t = 1$. By this solution it appears, that there are two angles at which the solid may be inclined from its upright position of unstable equilibrium with the flat surface upward, so as to rest permanently on the surface of the fluid, when that surface passes through one extremity of the base: 1st, when the angle of inclination is KDC $= 26° 33', 51'', 4$, or about $26° 34'$, of which the tangent is to radius as 1 to 2; and secondly, (fig. 8.) when the angle of inclination KDC $= 45°$, of which the tangent is equal to the radius. When the solid floats permanently on the fluid at the angle of inclination KDC $= 26° 34'$ from the upright position, the part immersed, or KCD, is to the whole volume ABCD as 1 to 4; and therefore the specific gravity of the solid is to that of the fluid as 1 to 4, or resuming the former notation applied to the present case, the specific gravity of the solid or $n = \frac{1}{4}$, when that of the fluid is $= 1$. That the position of equilibrium here determined is that of stability, appears from attending to the limiting value of the specific gravity, determined in page 69, where it is shewn that when the square parallelopiped is placed on the surface of a fluid with one of the flat surfaces horizontal, and the specific gravity of the solid is greater than .211, so as not to exceed .789, the equilibrium will be that of instability, and consequently the solid will overset. It has

been just shewn, that after the body has revolved through an angle of 26° 34′ it will be again in a position of equilibrium, which must therefore be the equilibrium of stability. Similar consequences follow from supposing the specific gravity $= \frac{1}{2}$; in this case if the solid is placed on the fluid with a flat surface upward, the equilibrium will be that of instability; and it appears from the preceding solution, that after revolving through an angle of 45°, (fig. 8.) it will again be in a position of equilibrium, which therefore will be stable and permanent. By a similar investigation, the angle of inclination ABK (fig. 9.) from the original position of equilibrium may be found when the solid floats permanently, and the fluid's surface intersects one of the extremities of the upper side of the square AB: for the notation remaining, by putting the tangent of the angle of inclination ABK $= t$, the area ABK $= \frac{a^2 t}{2}$, area KCDB $= \frac{2a^2 - a^2 t}{2}$, wherefore the specific gravity or $n = \frac{2 - t}{2}$; which quantity being substituted for n in the equation $\frac{t^2}{1 + t^2}$ * $= \frac{12n - 12n^2 - 2}{12n - 12n^2 - 1}$, there will arise the equation $\frac{t^2}{1 + t^2} = \frac{6t - 3t^2 - 2}{6t - 3t^2 - 1}$, exactly the same as in the former case; and by solving this equation it appears that $t = \frac{3}{4} \pm \frac{1}{4}$, and consequently the specific gravity of the solid, or $n = \frac{2 - t}{2} = \frac{3}{4}$ or $n = \frac{1}{2}$.

The only inquiry remaining to complete the investigation respecting the floating positions of the square parallelopiped, is to ascertain in what position the solid will float permanently

* Because s being the sine, and t being the tangent of the angle ABK, it follows that $s^2 = \frac{t^2}{1 + t^2}$.

with a plane angle obliquely upward, when the specific gravity is between the limits $\frac{8}{32}$ and $\frac{9}{32}$, or between the limits $\frac{23}{32}$ and $\frac{24}{32}$. It has been seen in a former investigation, that if the solid is placed on the fluid with an angle upward, and the specific gravity is $\frac{9}{32}$, it will just begin to float with stability, and ceases to float with stability when the specific gravity exceeds $\frac{23}{32}$. When the specific gravity is $\frac{1}{4} = \frac{8}{32}$ or $\frac{3}{4} = \frac{23}{32}$, it floats permanently with the surface of the fluid coincident with an extremity of one of the sides: if, therefore, the specific gravity is between the limits $\frac{8}{32}$ and $\frac{9}{32}$, or between $\frac{23}{32}$ and $\frac{24}{32}$, the solid will float permanently, with the diagonal line inclined to the vertical. This angle may be determined by finding an equation which expresses the relation between the given specific gravity and the sine or tangent of the required angle to radius $= 1$. Let a square parallelopiped IVCF (fig. 10.) float with an angle obliquely upward, so that the diagonal line shall make an angle with the vertical; suppose that angle to be OGT, the line GT being perpendicular to the horizon; let the surface of the fluid coincide with the line DE perpendicular to GT; take CB a mean proportional between EC and CD, and draw BA parallel to GV, intersecting the line GC in H; so shall CH be the depth to which the solid sinks in the fluid when the diagonal line CI is vertical, and consequently the area BXE is equal to the area XDA; take $CO = \frac{2}{3}$ CH; O will be the centre of gravity of the volume ABC; bisect EB in K, and AD in B; draw XR and XK; and take $XM = \frac{2}{3}$ of XR, and $HL = \frac{2}{3}$ of XK; M will be the centre of gravity of the

triangle XAD, and L will be the centre of gravity of the triangle BXE; through the points M, L, draw the lines MP, QL, perpendicular to the horizontal line DE; make PQ $= b$, the sine of BXE $= s$; the tangent of BEX $= t$ to radius $= 1$, and let EC $= a$.

Then CD $= ta$; and CB $= \sqrt{ta^2}$; CH $= \sqrt{\frac{ta^2}{2}}$; CO $= \frac{2}{3} \times$ CH $= \sqrt{\frac{2ta^2}{9}}$; the area ABC $= \overline{CH}^2 = \frac{ta^2}{2}$; put the area BXE $= u$; then to find the distance OT, the following proportion is to be made; as the area CDE or ABC is to the area BXE : : so is PQ* to OT; or as $\frac{ta^2}{2} : u : : b :$ OT $= \frac{2bu}{ta^2}$; and OG $= \frac{2bu}{ta^2 s}$; and since CO $= \sqrt{\frac{2ta}{9}}$, it follows that CG $= \frac{2bu}{ta^2 s} + \sqrt{\frac{2ta^2}{9}}$; and therefore CV $= \frac{\sqrt{8} \times bu}{ta^2 s} + \sqrt{\frac{4ta^2}{9}} = \frac{\sqrt{72} \times bu + \sqrt{4t^3 a^6 s^2}}{3ta^2 s}$; and the specific gravity being $= n$, $\sqrt{n} = \frac{\text{CH}}{\text{CV}} = \sqrt{\frac{ta^2}{2}} \times \frac{3ta^2 s}{\sqrt{72} \times bu + \sqrt{4t^3 a^6 s^2}} = \frac{3t^{\frac{3}{2}} a^3 s}{12bu + 2\sqrt{2}\, t^{\frac{3}{2}} a^3 s}$: thus, if the angle at which the diagonal line IC is inclined to the vertical line TN or OGT $=$ BXE should be 15°, the angle XEC $= 30°$; wherefore in the preceding expression, $t =$ tangent 30° to radius 1; $s =$ sine 15°; if CE or a is assumed $= 1$, on making the proper trigonometrical computations, the area BXE $= u = .039395$, and PQ $= b = 0.73089$; from substituting these quantities for their values in the equation $\sqrt{n} = \frac{3t^{\frac{3}{2}} a^3 s}{12bu + 2\sqrt{2}\, t^{\frac{3}{2}} a^3 s}$, it appears that $\sqrt{n} = \frac{.34063}{.3455^2 + .32114} =$ 0.51094, and $n = 0.261$ the specific gravity which causes the solid to float on the fluid in a position of equilibrium with a

* Page 59.

diagonal line obliquely upward, being inclined to the vertical at an angle of 15°; the equilibrium is that of stability, because when the diagonal is vertical, the solid floats in a position of unstable equilibrium, the specific gravity c.261 being less than $\frac{9}{32}$ or .281, the limiting value which separates the cases of permanent and unstable equilibrium when the solid is placed on the fluid with a diagonal line vertical.

It is curious to observe the conclusions which arise in the extreme case when the angle of inclination from the vertical is assumed = o; and consequently the angle XEC = 45°; for in this case CB = CE = a; $t = 1$; and BH = $\frac{a}{\sqrt{2}}$; therefore u or the area BXE * = $\frac{\overline{BH}^2 \times s}{2} = \frac{sa^2}{4}$; and since b = PQ = $\frac{4a}{3\sqrt{2}}$, it follows that $bu = \frac{a^3 s}{3\sqrt{2}}$; and $12bu = \frac{4a^3 s}{\sqrt{2}} = \sqrt{2}a^3 s$; which quantities being substituted for their values, the equation $\sqrt{n} = \frac{3t^{\frac{3}{2}} a^3 s}{12bu + 2\sqrt{2t^{\frac{3}{2}} a^3 s}}$ will become $\sqrt{n} = \frac{3a^3 s}{2\sqrt{2a^3 s} + 2\sqrt{2a^3 s}} = \frac{3}{4\sqrt{2}}$; and therefore $n = \frac{9}{32}$, agreeing † precisely with the specific gravity inferred by a different method from the same data.

The equation $\sqrt{n} = \frac{3t^{\frac{3}{2}} s}{2\sqrt{2t^{\frac{3}{2}} s} + 12bu}$ (the line CE = a being assumed = 1) expresses the relation between the specific gravity of the solid and the fraction representing the sine of the angle of inclination from the upright position: if, therefore, that

* Because the point of intersection X coincides with H when the angle BXE vanishes.

† Page 72.

angle is given, the specific gravity will be known. If it should be required to find the sine of the angle of inclination from having given the specific gravity, it is evident from the nature of the equation, that such determination would require analytical operations extremely complex and troublesome, which may be avoided by having recourse to well known methods of approximation. By assuming the quantities s and t by estimation, let the value of $\sqrt{n}$ be calculated from the equation, which being compared with the given value of $\sqrt{n}$, the difference will be the error arising from the error in the assumed values of s and t, which are therefore to be corrected, and the operation repeated until the value of $\sqrt{n}$, deduced from calculation, coincides with its true value; from which method of proceeding, the angle of inclination from the original position of equilibrium will be known.

This solution is evidently applicable to all cases in which the specific gravity of the solid is between the limits $\frac{8}{32}$ and $\frac{9}{32}$, and by an investigation entirely similar, an equation is deduced expressing the relation of the specific gravity of the solid and the sine or tangent of the angle of inclination from the perpendicular, when the specific gravity of the solid is between $\frac{23}{32}$ and $\frac{24}{32}$; in which case the solid will float permanently with the diagonal line IC obliquely upward, being inclined to the vertical at some angle between the limits 0 and 18° 26′ 8″,6.

These determinations comprehend all the positions in which a square parallelopiped can be placed on the surface of a fluid in a position of equilibrium, provided the solid is moveable only round one axis, namely, that which passes through the centre

of gravity perpendicular to the planes of the square sections; and this condition is insured by making the axis of sufficient length; for instance, if it is two or three times longer than one of the sides, the solid will not spontaneously revolve on any other axis. When the axis is diminished considerably, it is certain the body will be spontaneously moveable round some other axis; but it is unnecessary to enter into a detail of multiplied instances, since the exposition of principles is the material object in disquisitions of this kind.

The various positions which the square parallelopiped assumes when floating freely on a fluid's surface depending on its specific gravity, are brought under one point of view in the following abstract, the line IK denoting the surface of the fluid in the figures from fig. 11 to fig. 24.

If the specific gravity of the solid should be between the limits 0 and $\frac{1}{2} - \sqrt{\frac{1}{4} - \frac{1}{6}}$, (fig. 11, 12, and 13.) that is, between 0 and c.211, the solid floats permanently on the fluid with a flat surface upward, and parallel to the horizon.

If the specific gravity is between the limits .211 and .25 (fig. 13, 14, and 15.), the solid floats permanently with a flat surface upward, but inclined to the horizon at sundry angles of which the limits are 0°, corresponding to the specific gravity .211 and 26° 34′, corresponding to the specific gravity .25.

If the specific gravity is between the limits $.25 = \frac{8}{32}$ and $\frac{9}{32}$, (Tab. IV. and Tab. V. fig. 15, 16, 17.) the solid floats with one angle only immersed under the fluid's surface, the diagonal line being inclined to the vertical at various angles depending on the specific gravity, the limits of which angles are 18° 26′, cor-

responding to the specific gravity .25 $= \frac{8}{32}$, and 0, corresponding to the specific gravity $\frac{9}{32}$.

When the specific gravity is increased beyond $\frac{9}{32}$, (fig. 17, 18.) the solid floats permanently with a diagonal line vertical, till the specific gravity becomes $= \frac{23}{32}$.

If the specific gravity is of any magnitude between $\frac{23}{32}$ and $\frac{24}{32}$ the solid floats with the diagonal line inclined to the vertical at sundry angles depending on the specific gravity, (fig. 18, 19, 20.) the limits of which angles are 0, corresponding to the specific gravity $\frac{23}{32}$, and 18° 26′ corresponding to the specific gravity $\frac{24}{32}$, three angles of the solid being immersed under the fluid's surface.

If the specific gravity is between the limits $\frac{24}{32}$ and .789, (fig. 20, 21, 22.) the solid floats with a flat surface upward, and inclined to the horizon at sundry angles depending on the specific gravity, the limits of which angles are 26° 34′ corresponding to the specific gravity $\frac{24}{32}$ or .75, and 0 corresponding to the specific gravity .789.

When the specific gravity is of any magnitude between .789 and 1, the solid floats permanently with a flat surface parallel to the horizon.

From these determinations we also collect that while the solid in question, floating on the fluid's surface, revolves round its longer axis through 360°, it passes through either 16 or 8 positions of equilibrium. If the specific gravity should be between the limits .211 and .281, or between the limits .719 and

.789, the number of those positions will be sixteen; of which eight will be positions of permanent, and the remaining eight positions of unstable equilibrium; these different species of equilibrium succeeding each other alternately while the solid revolves round its axis. If the specific gravity should be of any value not included within these limits, the solid in revolving through 360° will pass through 8 positions of equilibrium only; of which four are positions of permanent, and four of unstable equilibrium.

In the investigations which have preceded, the solid is supposed to be of uniform figure in respect to the axis of motion, so as to make all the vertical sections drawn perpendicular to the axis equal. But when the floating body is of such a form that the sections drawn through it perpendicular to the axis at various points thereof are unequal, a different process, depending however on the same principles, will be necessary; both for determining whether the solid will float permanently or overset, and for ascertaining the several positions in which it will float on the surface of a fluid.

Let EFCD (fig. 23.) represent a cylinder* placed on the surface of a fluid with the axis NP vertical. Suppose the specific gravity to be such as causes the solid to sink to the depth QP; let it be required to determine in what cases, depending on the dimensions and specific gravity of the cylinder, it will float permanently in that position, and in what cases it will overset. Put the radius QA = r; the specific gravity of the solid = n, that of

* In this and the following propositions, the plane surfaces which terminate the solid are always understood to be perpendicular to the axis.

the fluid being $= 1$; let the centre of gravity be G; the centre of gravity of the immersed part $=$ O; GO $= d$; let AIBHSA represent a circular section of the cylinder coincident with the fluid's surface; draw any diameter IS; and a diameter AB perpendicular to IS; let the axis passing through the centre of gravity round which the cylinder is moveable be parallel to IS; through any point W of the diameter IS draw the ordinate KW perpendicular to IS, and produce KW till it intersects the circle in the point H; make QW $= z$; NP $= l$; $\pi = 3.14159$. It appears from page 66 that the solid will float permanently in the given position of equilibrium with the axis vertical, when the fluent of $\frac{\overline{KH}^3 \times \dot{z}}{12V}$ is greater than d, the letter V signifying the volume immersed under the fluid's surface; it is also shewn in page 66, that if d is greater than $\frac{\text{fluent of } \overline{KH}^3 \times \dot{z}}{12V}$, the equilibrium will be unstable; when the fluent of $\frac{\overline{KH}^3 \times \dot{z}}{12V} = d$, the equilibrium will be the limit separating the cases in which the solid floats with stability from those in which it is momentary and unstable. To ascertain the limit in the present case it is necessary to find the fluent of $\frac{\overline{KH}^3 \times \dot{z}}{12V}$. Since QS $= r$, and QW $= z$, WH $= \sqrt{r^2 - z^2}$, KH $= 2 \times \sqrt{r^2 - z^2}$, and $\overline{KH}^3 \dot{z} = 8 \times \overline{r^2 - z^2}^{\frac{3}{2}} \times \dot{z}$; the fluent of which quantity, while z increases from o to r is $\frac{8 \times 3\pi r^4}{16}$,* and for both semicircles, the

* Fluent of $\overline{r^2 - z^2}^{\frac{3}{2}} \dot{z}$ = fluent of $r^2 \times \overline{r^2 - z^2}^{\frac{1}{2}} \dot{z}$ − fluent of $\overline{r^2 - z^2}^{\frac{1}{2}} z^2 \dot{z}$.

Fluent of $r^2 \times \overline{r^2 - z^2}^{\frac{1}{2}} \times \dot{z} = r^2 \times$ the area QBHW. (fig. 23.)

fluent of $\overline{KH}^3 \times \dot{z} = 3\pi r^4$; and because PQ $= ln$, and the area of the circle AIBHSA is πr^2, the volume of the part immersed V is $= \pi r^2 ln$; moreover GP $= \frac{l}{2}$; and OP $= \frac{ln}{2}$; wherefore GO $= \frac{l - ln}{2} = d$: and since the $\frac{\text{fluent of } \overline{KH}^3 \times \dot{z}}{12V} = \frac{3\pi r^4}{12\pi r^2 ln}$; making $\frac{\text{fluent of } \overline{KH}^3 \times \dot{z}}{12V} = d$, in order to obtain the limit or limits which separate the cases of permanent and unstable equilibrium, we obtain the equation $\frac{3\pi r^4}{12\pi r^2 ln} = \frac{\overline{l - ln} \times}{2}$ or $\frac{r^2}{2l^2} = n - n^2$; $n^2 - n = -\frac{r^2}{2l^2}$; or if $2r$ is put $= b =$ the diameter of the base, $n^2 - n = \frac{-b^2}{8l^2}$ and $n = \frac{1}{2} \pm \sqrt{\frac{1}{4} - \frac{b^2}{8l^2}}$.

If therefore the diameter of the base bears to the axis a greater proportion than that of $\sqrt{2}$ to 1, no value can be given to the solid's specific gravity, which will cause it to float in a state of insensible equilibrium; or in other words, there is no specific gravity separating the cases in which the cylinder will float permanently, from those in which it will overset when the

$-$ Fluent of $\overline{r^2 - z^2}^{\frac{1}{2}} z^2 \dot{z} = \frac{r^2 z^2}{8} \times \sqrt{\frac{r^2 - z^2}{z^2}} - \frac{2z^4}{8} \times \sqrt{\frac{r^2 - z^2}{z^2}} + \frac{r^4}{8} \times \frac{\text{arc HS}}{r}$.

This quantity ought to be $= 0$, when $z = 0$; wherefore the entire fluent of $\overline{r^2 - z^2}^{\frac{3}{2}} z = r^2 \times$ area QBWH $+ \frac{r^2 z^2}{8} \times \sqrt{\frac{r^2 z^2}{z^2}} - \frac{2z^4}{8} \times \sqrt{\frac{r^2 - z^2}{z^2}} + \frac{r^4}{8} \times \frac{\text{arc HS}}{r} - \frac{\pi r^4}{16}$, because the arc $\frac{\text{HS}}{r} = \frac{\pi}{2}$ when $z = 0$, or SH $=$ SB; when $z =$ r, this fluent, that is, the fluent of $\overline{r^2 - z^2}^{\frac{3}{2}} \dot{z}$ while z increases from 0 to r is $=$ $r^2 \times$ area SBQ $- \frac{\pi r^4}{16} = \frac{\pi r^4}{4} - \frac{\pi r^4}{16} = \frac{3\pi r^4}{16}$.

axis is placed vertically; the cylinder, under these circumstances, must always float permanently with its axis vertical.

When the diameter of the base bears to the length a less proportion than that of $\sqrt{2}$ to 1, two values of the specific gravity may always be assigned, which will be the limits of the cases in which the solid floats with stability or oversets; *i. e.* $n = \frac{1}{2} \pm \sqrt{\frac{1}{4} - \frac{b^2}{8l^2}}$. If the specific gravity should be given, the proportion of the cylinder's length to the diameter of the base may be defined which limits the cases of stability or instability of floating with the axis vertical; for since $n - n^2 = \frac{b^2}{8l^2}$, it follows that $\frac{b}{l} = \sqrt{8n - 8n^2}$; consequently n being given, if the diameter of the base should be to the length of the axis in a greater proportion than that of $\sqrt{8n - 8n^2}$ to 1, the solid will float permanently with the axis upward; but if the base should be to the length of the axis in a less proportion than that of $\sqrt{8n - 8n^2}$ to 1, the solid will overset. Thus if n* $= \frac{3}{4}$, $\sqrt{8n - 8n^2} = \sqrt{\frac{3}{2}} = 1.2247$; if therefore the diameter of the base should be in a greater proportion to the length of the axis than 1.2247 to 1, it will float permanently with the axis vertical, if in a less proportion, it will overset from that position.

Suppose a parabolic conoid CEDK (fig. 24.) of given dimensions and specific gravity, should be placed on the surface of a fluid with the vertex downward, and the axis vertical; to ascertain the limits (depending on the length of the axis, the parameter of the parabola from which the conoid is formed, and the specific gravity,) which separate the cases in which the solid

* Page 48.

will float permanently with the axis vertical, or will overset, the plane of the base being supposed perpendicular to the axis: Let CED represent a plane section of this solid passing through the axis, which section will therefore be a parabola. Suppose the specific gravity to be such as causes the solid to sink to the depth FE. AIBHA represents a circular section of the solid which coincides with the fluid's surface; draw any diameter HI, and the diameter AB perpendicular to HI. Through any point W, in the radius FH, draw the ordinate KM perpendicular to FH; and suppose the solid to be moveable round an axis of motion parallel to the diameter HI; put the parameter of the parabola $= p$; the length of the axis KE $= a$, FW $= z$; the specific gravity $= n$; $\pi = 3.14159$; also let G be the centre of gravity of the solid, and O the centre of gravity of the part immersed. Then, since the volume immersed AEB is to the volume CED as $\overline{AB}^2 \times EF$ is to $\overline{CD}^2 \times EK$, or as $\overline{EF}^2$ to $\overline{EK}^2$; and since the volume immersed AEB is to the volume CED as n to 1, it follows that as $\overline{EF}^2 : \overline{EK}^2 = a^2 : : n$ to 1, and therefore $EF = a\sqrt{n}$, and $\overline{FB}^2 = pa\sqrt{n}$; referring to the expression for determining the stability of floating bodies when the inclinations from a position of equilibrium are very small, or $\frac{\text{fluent of } \overline{KM}^3 \dot{z} \times s}{12V} - ds$, we have, applicable to the present case, the entire fluent of $\overline{KM}^3 \dot{z} = 3\pi \times \overline{FB}^4$; or, because $\overline{FB}^4 = p^2 a^2 n$, the fluent of $\overline{KM}^3 \dot{z} = 3\pi p^2 a^2 n$: V or the volume immersed $= \frac{\pi a^2 pn}{2}$; and since by the properties of the figure, $GE = \frac{2a}{3}$ and $OE = \frac{2a\sqrt{n}}{3}$, we have $GO = \frac{2a - 2a\sqrt{n}}{3} = d$, these substitutions being made in the general value, $\frac{\text{fluent of } \overline{KM}^3 \dot{z} \times s}{12V} - ds$; this quantity becomes $= \frac{3\pi p^2 a^2 n \times 2 \times s}{12 \times \pi a^2 pn}$

$-\frac{\overline{2a-2a\sqrt{n}\times s}}{3}$, which being put $=$ o, in order to obtain the limiting value required, we obtain $\frac{p}{2}-\frac{2a-2a\sqrt{n}}{3}=\frac{3p-4a-4a\sqrt{n}}{6}=$ o, and $\sqrt{n}=\frac{4a-3p}{4a}$; consequently $\sqrt{n}:1::a-\frac{3p}{4}:a$.

From this determination it appears, that if the axis should be to the parameter in a proportion less than that of 3 to 4, no specific gravity can be given to the solid which will make it float in the equilibrium, which is the limit between the stability and instability of floating; secondly, if the specific gravity of the solid bears a greater proportion to that of the fluid than the proportion which the square of the difference between the axis and $\frac{3}{4}$ of the parameter bears to the square of the axis; when the axis is placed vertical, the solid will float with stability in that position; and thirdly, if the specific gravity of the solid bears a less proportion to the specific gravity of the fluid than that which the square of the aforesaid difference bears to the square of the axis, the solid will overset when placed on the fluid with the axis vertical, and will settle permanently with the axis inclined to the vertical line. These limits agree precisely with those which are demonstrated by ARCHIMEDES, in the second book of his tract, intituled *De iis quæ in humido vehuntur*,* prop. iii. and prop. iv.

* The demonstrations of ARCHIMEDES, which relate to the parabolic conoid, are founded on a supposition that this solid is generated by the revolution of a rectangular parabola on its axis; that is, of a parabola which is the section of a rectangular cone; in which case the line, called by the author (or rather by his translator, the original of this treatise being lost) "ea quæ usque ad axem," is half the principal parameter, being equal to the perpendicular distance between the plane which touches the cone, and the plane parallel to it, which is coincident with the parabola. This solid is termed by ARCHIMEDES, "conois rectangula," but the limitation appears to be unnecessary, because the demonstrations of the author are equally applicable to a solid generated by

If the specific gravity of the parabolic conoid should be less than the limit which has just been investigated, and if the axis should be to the parameter in a proportion greater than that of 3 to 4, and less than that of 15 to 8, it will float permanently on the fluid with the axis inclined to the horizon, and with the base wholly extant above the surface at some angle less than 90°; which angle may be determined by the following geometrical construction, subject to the limitation which will appear from the construction itself, or rather from the computation founded upon it.

Let ASBTD (Tab. VI. fig. 25.) represent a section of the parabolic conoid which passes through the axis; which section will be a parabola. Let the axis BE be divided into three equal parts, one of which is EF. By the properties of this figure, F will be the centre of gravity of the solid. In the line FB take FH equal to half of the parameter, and through H draw the indefinite line ΓGZ perpendicular to BE, and in the line GZ take HK = FB; in the line HΓ take HI, which shall be to HK in the proportion of the specific gravity of the solid to that of the fluid; and bisect IK in the point L; with the centre L and radius LI describe the semicircle KOI, intersecting the axis BE in the point O; through O draw OC parallel to KI, intersecting the parabola in the point C, and let PCN be drawn touching the parabola in the point C. Through C draw the indefinite line CR parallel to BE, intersecting the line KI in the point G; in

the revolution of a parabola, which is the section of any cone, whatever may be the angle at the vertex, half the parameter being substituted instead of the line, called by ARCHIMEDES "ea quæ usque ad axem;" and it is a property of conics easily demonstrable, that any parabola being given, a similar and equal parabola may be formed from the section of any cone, whatever may be the angle at the vertex, the axis being of sufficient length.

the line CR take GQ equal to half GC; and through Q draw SQT parallel to PCN. When the conoid floats permanently and at rest, the surface of the fluid will coincide with the line SQT, and the axis will be inclined to the horizon at the angle ONC: through the points F and G draw the indefinite line FGM.

The order of the demonstration will be as follows. First, to shew that, according to the construction, the volume of the immersed part SCBT is to the whole magnitude of the solid in the proportion which the specific gravity of the solid bears to that of the fluid: secondly, to shew that the centre of gravity of the solid and the centre of gravity of the part immersed are in the same vertical line; and consequently the construction will place the solid in a position of equilibrium: thirdly, to demonstrate that the equilibrium so constituted is that of stability.

Since by the properties of the circle, HI is to HK as the square of HO is to the square of HK; and the square of HO is to the square of HK as the square of CQ ($= \frac{3}{2} \times$ HO) is to the square of BE ($= \frac{3}{2}$ BF): therefore, since by the construction the specific gravity of the solid is to that of the fluid as HI to HK, it follows, that as the specific gravity of the solid is to the specific gravity of the fluid, so is the square of CQ to the square of BE: but by the properties of the parabolic conoid the magnitude of the segment SCBT is to the magnitude of the whole solid ACBTD as the square of CQ to the square of BE; and consequently it is proved that when the solid floats according to the position described in the construction, the volume immersed SCPT will be to the whole magnitude as the specific gravity of the solid is to that of the fluid, which was in the first place to be de-

monstrated. Secondly, because CQ is the abscissa of the segment SCT corresponding to the vertex C and ordinate SQ, and by the construction CG = 2 GQ, it follows from the properties of the solid that G is the centre of gravity of the segment or part immersed SCBT. By the properties of the parabola, as ON is to CO so is CO to half the parameter, that is, as ON : CO :: CO = GH : FH; therefore since the triangles GHF, CON, have one right angle each, and the sides round the equal angles are proportional, the triangles will be similar; consequently the angle OCN = the angle NFG: the sum of the angles FNC, NFC, is therefore a right angle, and the line FGM is perpendicular to the horizontal line PCN; and since F by construction is the centre of gravity of the parabolic conoid, and G has been proved to be the centre of gravity of the part immersed, and the line FGM is vertical, it follows, that the centres of gravity of the entire solid and of the part immersed are in the same vertical line, and consequently the solid is in a position of equilibrium, according to the construction. Thirdly, this equilibrium is that of stability; for let the solid be conceived to be turned round an axis passing through the centre of gravity, through a small angle, in such a direction as to depress the parts towards D, and to elevate those near to A; in that case the lowest point of the curve will be situated between C and B; suppose it to be at W, draw WX = CQ, parallel to BE, and take Wg = $\frac{2}{3}$ of WX. Then since* $\overline{CQ}^2$ is to $\overline{BE}^2$ as the specific gravity of the solid to that of the fluid, it is evident that however the axis BE is inclined to the horizon, $\overline{CQ}^2$ and consequently CQ must

* Page 98.

always continue of the same value, and therefore $\frac{2}{3}$ of CQ $= \frac{2}{3}$ of WX or CG $=$ Wg; consequently g is the centre of gravity of the part immersed after the inclination. And since the abscissa or portion of the diameter intercepted between the lowest point and surface of the fluid must always be of the same magnitude while the specific gravity remains the same; and by the construction Wx is made equal to the abscissa CQ; it follows, that when the solid has been so inclined, that the lowest point shall coincide with W, CG $= wg$, and consequently wg is always less than wV; if therefore a line gz is drawn through the centre of gravity g perpendicular to the horizon, the point of intersection z with the horizontal line RU will be between the points F and U; and the pressure of the fluid acting in the direction of the line gz will cause an angular motion in the solid,* which elevates the point D and depresses the point A, or, in other words, will counteract the inclination of the solid, by which it is deflected from its position of equilibrium. By the same method of argument it is shewn, that if the solid is inclined on the contrary direction, a force is created by the position of the centre of gravity of the part immersed, which restores the solid to its former situation, as found by the construction; which therefore places the solid in a position of equilibrium which is permanent.

The several conditions by which this construction is limited will be more easily deduced from analytical investigation, than from having recourse to geometrical constructions.

To represent in general terms the angle CNO, at which the axis of the solid is inclined to the horizon, let BE $= a$; 2HF or the parameter $= p$; also let the specific gravity of the solid be to that of the fluid as n to 1; consequently

* Page 63.

$FH = \frac{p}{2}$; $FB = \frac{2a}{3}$; $BH = \frac{2a}{3} - \frac{p}{2} = \frac{4a - 3p}{6}$; and since by the construction $KH : HI : 1 : n$, and $KH = BF = \frac{2a}{3}$, it follows that $HI = \frac{2an}{3}$, and $HO = \frac{2a\sqrt{n}}{3}$; consequently $OB = HB - HO = \frac{4a - 3p}{6} - \frac{2a\sqrt{n}}{3} = \frac{4a - 3p - 4a\sqrt{n}}{6}$; $CO = \sqrt{\frac{4a - 3p - 4a\sqrt{n}}{6}} \times \sqrt{p}$: and because $ON = \frac{4a - 3p - 4a\sqrt{n}}{3}$, it appears that CO will be to ON, that is, the tangent of the angle of inclination CNO, will be to radius as $\sqrt{\frac{4a - 3p - 4a\sqrt{n}}{6}} \times \sqrt{p}$ to $\frac{4a - 3p - 4a\sqrt{n}}{3}$, or as $\sqrt{\frac{\frac{3}{2} \times p}{4a - 3p - 4a\sqrt{n}}}$ to 1. When therefore the angle CNO becomes equal to 90°, that is, when the solid floats with the axis in a vertical position, the tangent of inclination $= \sqrt{\frac{\frac{3}{2} p}{4a - 3p - 4a\sqrt{n}}}$ becomes infinite, or, which is the same thing, $4a - 3p - 4a\sqrt{n} = 0$, and consequently $\sqrt{n} = \frac{4a - 3p}{4a}$, precisely coinciding with the limit deduced by a different method * of investigation.

But another inquiry is here suggested. It is evident that this construction is applicable only while the solid floats in such a manner that the whole of the base AD shall be extant above the fluid's surface. To know in what cases this condition takes place, it will be necessary to investigate what must be the value of the solid's specific gravity, and the proportion of the axis to the parameter when the solid floats permanently, so that the surface of the fluid shall pass through one of the extremities of the base A. The result will shew the limit, or limits, if there are more than one, which sepa-

* Page 96.

rate the cases in which the solid floats permanently with the base entirely extant above the fluid's surface, from those in which a part of the base is immersed under it.

The notation remaining as before, since OB or BN $= \frac{4a - 3p - 4a\sqrt{n}}{6}$ and EB $= a$, (fig. 26.) by addition EN $= \frac{10a - 3p - 4a\sqrt{n}}{6}$; and because NW $=$ CQ* $= a\sqrt{n}$, it follows that EW $=$ EN $-$ NW $= \frac{10a - 3p - 10a\sqrt{n}}{6}$: and since AE $= \sqrt{ap}$, the tangent of the angle CNO or AWE, is to radius, as EA to EW, or as $\sqrt{ap} : \frac{10a - 3p - 10a\sqrt{n}}{6}$, that is, making the radius $= 1$, the tangent of the angle AWE or CNO $= \frac{\sqrt{ap} \times 6}{10a - 3p - 10a\sqrt{n}}$; but the tangent† of CNO $= \sqrt{\frac{\frac{3}{2} \times p}{4a - 3p - 4a\sqrt{n}}}$, which two quantities are therefore equal, or $\frac{\sqrt{ap} \times 6}{10a - 3p - 10a\sqrt{n}} = \sqrt{\frac{\frac{3}{2}p}{4a - 3p - 4a\sqrt{n}}}$; or if $1 - \sqrt{n}$ is put $= m$, $\frac{\sqrt{ap} \times 6}{10ma - 3p} = \sqrt{\frac{\frac{3}{2} \times p}{4ma - 3p}}$; and by squaring both sides, $\frac{36ap}{100m^2a^2 - 60map + 9p^2} = \frac{3p}{2 \times 4ma - 3p}$, or $\frac{24a}{100m^2a^2 - 60map + 9p^2} = \frac{1}{4ma - 3p}$, which is reduced to the equation, $100m^2a^2 - 60m\,ap + 9p^2 = 96m\,a^2 - 72ap$; or $m^2 - \frac{60pa + 96a^2}{100a^2} \times m = \frac{-9p^2 - 72ap}{100a^2}$. Wherefore $m = \frac{30pa + 48a^2}{100a^2} \pm \sqrt{\left(\frac{30ap + 48a^2}{100a^2}\right)^2 - \frac{9p^2 + 72ap}{100a^2}} = \frac{30p + 48a}{100a}$

* By the preceding investigation in appears, that HO $= \frac{2a\sqrt{n}}{3}$, and since HO $=$ GC and GQ $= \frac{1}{2}$ GC, it follows that CQ or NW $= a\sqrt{n}$.

† Page 101.

$\pm \sqrt{\frac{576a^2 - 1080ap}{2500a^2}} = \frac{15p + 24a \pm \sqrt{576a^2 - 1080pa}}{50a}$; consequently, restoring the value of $m = 1 - \sqrt{n}$, $1 - \sqrt{n} = \frac{15p + 24a \pm \sqrt{576a^2 - 1080pa}}{50a}$; and therefore $\sqrt{n} = \frac{26a - 15p \pm 6\sqrt{2a} \times \sqrt{8a - 15p}}{50a}$.

Various inferences follow from this determination. In the first place, although the object of the preceding investigation was, to find a single value only of the specific gravity, which would cause the solid to float permanently with the extremity of the base coincident with the fluid's surface, yet by the result it appears, that there are two values of the specific gravity which will answer this condition under a certain limitation, which is also discovered by the solution; this is, that the axis (a) shall be to the parameter (p) in a proportion greater than that of 15 to 8; for if that proportion should be less, $8a$ will be less than $15p$; in which case the quantity $\sqrt{8a - 15p}$ becomes impossible. From which circumstance it may be inferred, that whenever the axis is to the parameter in a less proportion than of 15 to 8, the solid will float permanently on the fluid with the whole of the base extant above the fluid's surface, whatever may be the specific gravity of the solid. This limit is precisely the same with that which is demonstrated by ARCHIMEDES, in the second book of his tract, intituled *de iis quæ in humido vehuntur*, prop. vi. When the axis bears a greater proportion to the parameter than that of 15 : 8, the solid will float either with the base entirely out of the fluid, or partly immersed under it, according to the specific gravity. Having given the axis a in a greater proportion to the parameter p than 15 to 8, by making the

specific gravity $n = \left\{\frac{26a - 15p + 6 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50a}\right\}^2$ or $n = \left\{\frac{26 - 15 - 6 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50a}\right\}^2$, the specific gravity of the fluid being $= 1$, the solid will float with the extremity of the base in contact with the fluid's surface. If the specific gravity is greater than $\left\{\frac{26a - 15p + 6\sqrt{2a} \times \sqrt{8a - 15p}}{50a}\right\}^2$, or less than $\left\{\frac{26a - 15p - 6 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50a}\right\}^2$, the solid will float with the base wholly above the surface. If the specific gravity of the solid is to that of the fluid in any proportion between the limits $\left\{\frac{26a - 15p + 6 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50}\right\}^2$ to a^2, and $\left\{\frac{26a - 15p - 6 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50}\right\}^2$ to a^2, the solid will float with the base partly immersed beneath the fluid's surface.

These limits are determined by geometrical construction in the treatise before quoted (lib. 11. prop. x. *et seq.*) to which construction the preceding investigation may serve as a comment and analysis; and some elucidation of this kind may perhaps be deemed the more requisite, since no traces are to be found in the work referred to of the method of investigation or train of reasoning, by which a problem of so much difficulty was solved, without assistance from analytical operations, at least from any that would seem competent to such an inquiry.*

* Before any proposition can be demonstrated synthetically, it must have been investigated or discovered by some previous train of reasoning: it has been supposed that the ancient geometricians purposely concealed the analysis of their propositions; but as no satisfactory evidence is produced to support this conjecture, it is probable that the supposed concealment arose from the want of a proper notation, by which analytical investigations might be conveniently expressed.

This construction of Archimedes* may be justly regarded as one of the most curious remains of the ancient geometrical synthesis, and is here inserted, in order that the agreement between the solutions by analytical investigation and geometrical construction, may appear in the most satisfactory point of view.

Having given the parabola APBL, (fig. 27.) which is a section of a conoid passing through the axis BD, and having given the axis BD, which is to the parameter in a greater proportion than 15 to 8, it is required to express, by geometrical construction, the two proportions which the specific gravity of the conoid must bear to that of the fluid, so that the solid may float permanently on the fluid when the surface passes through an extremity of the base.

BD represents the axis of the conoid, DA is the greatest ordinate to the axis; join the points B and A, and bisect BA in T; draw TH perpendicular to AD; and with the axis TH, and ordinate AH, describe the parabola ATD; in the axis BD set off DK $= \frac{1}{3}$ of DB, and make KR $= \frac{1}{2}$ the parameter; also set off KC to DB in the proportion of 4 to 15: consequently DB bears a greater proportion to KR than 15 to 4; and since KR is half the parameter, it follows that the axis is to the parameter in a greater proportion than that of 15 to 8. Through C draw CE parallel to DA intersecting BA in E, and draw EZ perpendicular to AD. With the ordinate AZ and axis ZE describe the parabola AEI, and through R draw the line RGY, intersecting the parabola AEI in the points G and Y; through the points G and Y draw the lines ON, PQ, perpendicular to AD, intersecting the parabola ATD in the points X and F.

* *De iis quæ in humido vehuntur*, Lib. ii. prop. x.

Then the proposition affirms that the solid will float permanently on the fluid with the surface thereof in contact with one extremity of the base, when the specific gravity of the solid is to that of the fluid as the square of the line OX is to the square of the axis BD, or as the square of the line PF is to the square of the axis BD.

Instead of inserting the geometrical demonstration of this construction, it will be more expedient, in the present instance, to proceed by a contrary method of argument, *i. e.* by assuming the construction as true, and inferring from it the proportions of the specific gravities in question, and comparing the proportions so inferred with those which have been already found by analytical investigation. Proceeding, according to this method, through the points X and O draw the lines SX, OY, parallel to AD; and since the axis $DB = a$, and $DK = \frac{a}{3}$ by construction, and $KC = \frac{4a}{15}$, it follows that $DC = \frac{9a}{15}$, and $BC = \frac{6a}{15}$; moreover, by the properties of the parabola $DA = \sqrt{ap}$, and the triangles ABD, ECB, being similar, $EC = \frac{DA \times BC}{BD} = \frac{\sqrt{ap} \times 6}{15} = ZD$; moreover, as DB : ZE or DC : : DA : ZA, that is, as $a : \frac{9a}{15} : : \sqrt{ap} : ZA = \frac{9\sqrt{ap}}{15}$; and consequently the parameter of the parabola $AEI = \frac{\overline{AZ}^2}{ZE} = \frac{\overline{AZ}^2}{DC} = \frac{9 \times 9 \times ap}{15 \times 15} \times \frac{15}{9a} = \frac{9p}{15}$. And because $RC = EM = KC - KR = \frac{8a - 15p}{30}$, and $\overline{ZN}^2 =$ parameter of the parabola AEI $\times$ ME, it follows that $ZN = \sqrt{\frac{ME \times 9p}{15}} = \sqrt{\frac{8pa - 15p^2}{50}}$, and $ND = ZD - ZN = EC - ZN = \frac{\sqrt{ap} \times 6}{15} - \sqrt{\frac{8ap - 15p^2}{50}} = \frac{\sqrt{8ap} - \sqrt{8ap - 15p^2}}{\sqrt{50}}$,

and $BY = \frac{ND^2}{p} = \frac{16a - 15p - 4\sqrt{2a} \times \sqrt{8a - 15p}}{50}$, and $YD = ON$

$= a - \frac{16a - 15p - 4\sqrt{2a} \times \sqrt{8a - 15p}}{50} = \frac{34a + 15p + 4\sqrt{2a} \times \sqrt{8a - 15p}}{50}$;

and since $HN = HD^* - ND$, we shall obtain $HN = HD$

$= HA = \frac{\sqrt{ap}}{2} - \frac{\sqrt{8ap} - \sqrt{8ap - 15p^2}}{\sqrt{50}} = \frac{\sqrt{ap} + \sqrt{16ap - 30p^2}}{10}$,

and $TS = \frac{\overline{HN}^2}{\text{parameter of ATD}} = \frac{17a - 30p + 2\sqrt{2a} \times \sqrt{8a - 15p}}{50}$;

wherefore since $TH = \frac{a}{2}$, and $NX = TH - TS$, it follows that $NX = \frac{a}{2} - \frac{17a - 30p + 2\sqrt{2a} \times \sqrt{8a - 15p}}{50} =$

$\frac{8a + 30p - 2\sqrt{2a} \times \sqrt{8a - 15p}}{50}$; or finally, $OX = ON - NX =$

$\frac{34a + 15p + 4 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50} - \frac{8a + 30p - 2 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50}$

$= \frac{26a - 15p + 6\sqrt{2a} \times \sqrt{8a - 15p}}{50}$.

By a computation similar to the preceding it is found, that the line $PF = \frac{26a - 15p - 6\sqrt{2a} \times \sqrt{8a - 15p}}{50}$.

It is therefore a consequence, from the geometrical construction assumed as true, that the parabolic conoid will float permanently with the extremity of the base in contact with the fluid's surface; if the specific gravity of the solid is to that of the fluid, either in the proportion of

$\left|\frac{26a - 15p + 6 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50}\right|^2$ to a^2, or in that of

$\left|\frac{26a - 15p - 6 \times \sqrt{2a} \times \sqrt{8a - 15p}}{50}\right|^2$ to a^2; precisely agreeing with the proportions which were deduced from analytical investigation:†

* $HD = HA = \frac{\sqrt{ap}}{2}$.

† Page 104.

by which agreement both the construction and investigation receive the most satisfactory confirmation.

It has been observed in the course of the preceding pages, that the theorems* investigated to discover the floating positions of bodies, are no less applicable to ascertain the stability of floating, or the resistance which the fluid's pressure opposes to any force applied to incline a floating body from its position of equilibrium. This latter branch of statics is a subject deserving of every attention which science and practical experience can bestow upon it, from the immediate relation it bears to the motion and equilibrium of ships at sea. By this principle, the wind's impulses become effectual in propelling vessels, which, in default of stability, are rather inclined from the perpendicular than moved forward by the force of the wind: and when a ship has been nearly overset by the violence of the elements, it is the power of stability which still sustains, and (if sufficient) at length restores it to the upright position.

The stability of a floating body when inclined through any angle from the perpendicular, has been obtained by investigating a general value of the perpendicular distance GZ† = $\frac{bA}{V} - ds$; (fig. 2.) for the distance between the two vertical lines, one of which passes through the centre of gravity of the solid, and the other through the centre of gravity of the volume immersed. This principle is now to be applied to ascertain the stability of ships: this will be effected by finding either by construction or by calculation, the length of the line GZ: and if the vessel's weight should be W, the measure of stability will be GZ × W, by which it is plainly seen, that if any force M should be applied at a distance from the centre of gravity SG, (fig. 2.) and in a direction perpendicular to SG, to balance

* Page 61. † Page 60.

or counterpoise the force of stability, there will arise the equation $M \times SG = W \times GZ$.

In the particular case, when the angles at which a floating solid is inclined from the position of equilibrium are very small, the line GZ (fig. 2.) has been found * $= \frac{\text{fluent of } \overline{AB}^3 \times \dot{z} \times s}{12V} - ds$: in which expression $\dot{z}$ is a small portion of a line drawn coincident with the fluid's surface, and parallel to the axis of motion; AB is the breadth of the solid at the water's surface, corresponding to the line z parallel to the axis; V is the total displacement or volume immersed; d is the distance GO; and s the sine of the small angle of inclination from the position of equilibrium. Respecting this expression it is observable, that since $\frac{\text{fluent of } \overline{AB}^3 \times s \times \dot{z}}{12V} = ET$, (fig. 2.) and $d = OG = EG$, it follows that $\frac{\text{fluent of } \overline{AB}^3 \dot{z}}{12V} = ES$, and $\frac{\text{fluent of } \overline{AB}^3 \dot{z}}{12V} - d = GS$; which quantity is invariably the same whatever may be the inclination of the floating body from the position of equilibrium, provided that inclination is very small; that is, the point S is immoveable in respect of the point G, while the floating body revolves through any different small angles round the axis, passing through the centre of gravity G in a direction perpendicular to the plane ADHB. Since, therefore, the measure of stability $GZ \times W$ is $\frac{\text{fluent of } \overline{AB}^3 \times \dot{z} s}{12V} - ds \times W$ and $\frac{\text{fluent of } \overline{AB}^3 \times \dot{z}}{12V} - d = GS$, (fig. 2.) it follows that the measure of stability $= W \times SG \times s$, agreeing with the value which EULER has deduced by other methods for expressing the stability of vessels when the angles of inclination are evanescent.†

* Page 66.

† *Théorie complette de la Construction des Vaisseaux*, chap. viii.

If SG = o, that is, if the centre of gravity of the solid coincides with the point of equipoise S, otherwise called the metacentre,* or centre of equilibrium, the stability will be = o, or in other words, the solid will float in all positions alike, without effort to restore the upright position when inclined, or to incline itself further; it being remembered that the angles of inclination are very small. When the centre of gravity is situated beneath the metacentre, the solid must always float with stability, the measure of which is W × SG × *s*, in which case this force acts on the solid to turn it in a direction contrary to that in which it is inclined from the upright position; but when the centre of gravity is placed above the metacentre, (fig. 2.) the quantity W × SG × *s* having passed through o, becomes a force which acts to turn the solid in the same direction in which it is inclined, and will therefore constitute the equilibrium of instability. The determination of the point S becomes, for these reasons, of consequence in estimating the stability of vessels and other bodies when the angles of inclination are very small, and is particularly of use in ascertaining whether a solid, when placed on a fluid in a given position of equilibrium, will float permanently in that position, or will overset. Because it depends on the stability or instability† of floating when the angles of inclination are of evanescent magnitude, whether the solid will continue to float in a position of equilibrium or will revolve on an axis until it settles in some other. These theorems, however, for the measure of stability being applicable only in those cases when the angles of inclination from the position of equilibrium are extremely small, when a ship or other body is inclined 10°, 15°, or 20°, the stability of floating is to be ob-

* BOUGUER. Liv. i. sect. iii. chap. iv. † Page 66.

tained by having recourse to the theorem demonstrated in page 59, where it is shewn, that the stability of a vessel is truly measured by its weight, and the distance between the two vertical lines which pass through the centres of gravity of the vessel and the centre of gravity of the immersed volume; or if s be put to represent the sine of the angle of inclination from the perpendicular, V = the total displacement or volume immersed; A = the volume immersed in consequence of the inclination; b = the horizontal line bc; d = the line GO, (fig. 2.) and W = the weight of the vessel, the measure of the vessel's stability appears by this theorem to be $W \times GZ = \frac{\overline{bA}}{V} - \overline{ds} \times W$. In applying this expression to any case in practice, it is supposed that the position of the centre of gravity of the ship, and the position of the centre of gravity of the immersed volume, when the ship floats in an upright position, are both known, and consequently the distance of those points, represented by the line GO = d, is a given or ascertained quantity. The total displacement is supposed to have been determined by previous measurements, which quantity is denoted by the letter V; and consequently the weight of a quantity of water, the volume of which is V, will be = W, or the vessel's weight. s, the sine of the angle of inclination from the upright position, is necessarily given from the nature of the case, and may be of any magnitude. The only quantity which remains to be determined, for ascertaining the measure of the vessel's stability, is bA. To facilitate this determination the following observations are premised. If a line be conceived to pass through the centre of gravity parallel to the horizon from the head to the stern, when the ship floats in an upright position, that line is termed the longer axis, to distinguish it from another line, also horizontal, which passes

through the centre of gravity in a direction perpendicular to the former, and is called the shorter or transverse axe. A vertical plane drawn through the longer axe when the vessel floats upright divides it into two parts perfectly similar and equal; in which particular the figures of ships may be termed regular; although in other respects they are of forms not restrained to any uniform proportions. From the equality of these two divisions of a vessel, it must necessarily happen that when it floats in a quiescent position the similar parts on the opposite sides will be equally elevated above the water's surface. A ship thus floating in a position of equilibrium may be conceived to be divided into two parts, by the horizontal plane which is coincident with the water's surface; and the section formed by this plane passing through the body of the vessel is termed the principal section of the water, and is represented in fig. 2. as coincident with the line AB: when the ship is caused to heel, by being inclined round the longer axe through any angle SGK or NXB, (fig. 2.) the plane in the ship represented by the line AB will be transferred to the position IN, and the section of the water will now pass through the vessel in the direction of a plane coincident with AP, inclined to the former plane in the angle NXP, and may be termed, merely for the sake of distinction, the secondary section of the water. These two planes intersect each other in the line denoted by the point X, or rather in the line which is projected into the point X on the plane ABDH. Since the vessel is supposed to be inclined round the longer axe, it follows, that the line of intersection denoted by X will be parallel to that axis. And since from the laws of hydrostatics the volume PXN, which has been immersed in consequence of the inclination, is equal to the volume IXW, which has been elevated

above the water's surface by the same cause are precisely equal, the position of the line represented by the point X (always parallel to the axis) will depend on the figure which is given to the sides of the vessel PN, WI. It has been seen that when the figure is a parallelopiped floating with two plane angles thereof immersed, the point X (fig. 6.) bisects the lines corresponding to AB or IN in fig. 2: when the same solid floats with one plane angle only immersed, (fig. 10.) the point X is removed nearer to those parts of the solid which are more immersed by the inclination. In a ship, the breadth of which continually alters from the head to the stern, and in no regular proportion expressible by geometrical laws, it is evident that the position of the point X, representing the line in which the water's surface intersects the vessel in its two positions, must be determined practically by methods of approximation, from which, at the same time, the other requisites for this solution will be obtained. Since to find the value of the quantity bA in the expression $W \times \sqrt{\frac{b\,A}{V}} - ds$, it is necessary that the position of the point X should previously be known: to determine this particular it will be expedient to conceive the volume (fig. 2. and 28.) NXP, which has been immersed in consequence of the inclination, and that which has been elevated above the fluid's surface, or IXW, to be divided into segments, by vertical planes passing perpendicular to the longer axis, and at a distance of a few feet from each other, for instance, 2 or 3 feet; each of these segments will be of a wedge-like form, (fig. 28.) contained between two planes, XxPp and XxNn, inclined to each other at the given angle of inclination NXP; two vertical parallel planes NXP, nxp, which are nearly equal, and the portion NPnp, of the ship's side.

The distance between the planes NXP, *nxp*, is the line $Xx = Nn = Pp$; Xx produced, is the line in which the two sections of the water intersect each other, and is therefore coincident with the water's surface, and is parallel to the longer axis. The dimensions of the vessel being supposed known, the lines AB, NI, will be known in fig. 2: from these data the lines NX, PX, (fig. 2. and 28.) are to be assumed by estimation, and the angle NXP being given by the supposition, the area NXP is known from the rules of trigonometry, and the area PTNP may be inferred by the known methods of approximation.*

In like manner the area *xptn* is to be determined, and a mean of the two areas being multiplied into the thickness or

* STIRLING. *De Interpolatione Serierum*, prop. xxxi. CHAPMAN. *Traité de la Construction des Vaisseaux*, ch. i.

Methods of approximating to the areas of curves, founded on the differential serieses, are given by several authors, particularly by STIRLING and SIMPSON. Admiral CHAPMAN proposes a very ingenious method of approximation, depending on the properties of the parabola; either is sufficiently exact for the purposes of practical geometry, as appears by the instance inserted underneath: but of the two methods that of Mr. STIRLING is the most correct. The two methods of approximation are severally applied and compared in the following example of finding the curvilinear area, which is comprehended between an arc of 30° and the radius, sine, and cosine of the said arc: to obtain this area by approximation, 5 equidistant ordinates are given; *i. e.* 1st. ordinate = radius = 8, 2d. = $\sqrt{63}$: 3d. = $\sqrt{60}$, 4th. = $\sqrt{55}$, 5th. = $\sqrt{48}$.

The approximate area is,

According to the method of STIRLING, -	30.61153	According to the method of CHAPMAN, -	30.61131
Correct area - -	30.61156		30.61156
Error of approximation —	.00003	—	.00025

The same method by which the areas of curves are found by approximation, may be applied with equal exactness to determine the solid contents of space, and the position of the centre of gravity.

distance Xx will be the solid contents of this segment, to a degree of exactness fully sufficient for the purposes of this approximation. In the same manner the solid contents of all the segments which are elevated above the surface are to be obtained by making XI = AB − NX, XW = AB − PX, and proceeding as in the former case. If the aggregate of the segments NXP representing the part immersed, in consequence of the vessel's inclination, should not be equal to the aggregate of the segments IAW, (fig. 2.) which are elevated above the surface, the position of the point X, or rather of the line which that point denotes, must be altered, and the same operations repeated till the sums of the segments on each side of the said line of inclination are precisely equal.

This having been effected, the magnitude of the volume immersed, denoted by A in the expression $W \times \frac{\overline{bA}}{V} - \overline{ds}$, will be known; and the magnitude of each of the individual segments NXPnxp and IXWixw, &c. will also be known; the quantity bA will be found in the following manner. The area PXNTP and its centre of gravity d are to be determined by methods of approximation. Through d draw dc perpendicular to the horizontal line PX; Xc* will be the

* The solution of problems by geometrical construction has been little practised since methods of calculation have been so much improved by the invention of logarithms and other facilities: the solutions of difficult cases are, however, sometimes obtained with sufficient exactness by construction, which would be more troublesome by any other method: in the present instance, after the area PTNP and its centre of gravity have been determined, the position of the centre of gravity d, of the entire area XNTP, and the length of the line Xc, may be most easily ascertained by the method of construction. If the line PN is bisected in the point C, the centre of gravity of the triangle PXN will be situated at the distance of $\frac{1}{3}$ CX from the point C: the centre of gravity of the triangle PXN being thus constructed with geometrical exactness, it follows, that the centre of gravity of the entire area PXNTP, which is the

distance of the centre of gravity *d* from the point X, estimated in the direction of the horizontal line PX.

The same operations being applied to the area *xptn*, will give the distance *ex* of the centre of gravity of the area *xptn*, from the point *x*, estimated in the direction of the horizontal line *px*; the mean of the two distances so found will be the distance of the centre of gravity of the solid segment XPN*xpn*, from the line X*x*, estimated in the direction of the horizontal line XP or *xp*, to a degree of exactness entirely sufficient for this approximation.

Similar distances of the centres of gravity of all the segments (fig. 2. and 28.) PXN*pxn*, corresponding to the line X*x* produced, having been found, also of all the segments IXW*ixw*, if each of these segments is multiplied into the distance of its centre of gravity from the line X*x*, estimated in a horizontal direction, the sum of the products so formed will be the value of the quantity *b*A in the expression $W \times \overline{\frac{bA}{V} - ds}$, which is the measure of the vessel's stability, when inclined from its upright position through an angle PXN of which the sine is to radius as *s* to 1: and the quantities* W, V, and *d*, having been previously determined, it is evident that from the methods which have been described, the vessel's stability when inclined to the given angle will be obtained.

It would be improper, in a disquisition not written on the practice of naval architecture, to enter into further detail on this subject. By what has preceded, it is evidently seen that the stability of vessels may be determined for any angles at which they are inclined from the position of equilibrium, as well as for those which are very small. In both cases

common centre of gravity of the areas PXN and PNTP, is capable of being determined with very great precision.

* Page 111.

it is necessary that the position of the centre of gravity of the ship, and that of the part immersed, when the ship floats upright, should be known; practical methods of mensuration are required, in both cases, to ascertain these points. When the angles of inclination are very small, to find the ship's stability, it is necessary to measure* the successive ordinates or breadths of the ship on a level with the water's surface, and when the angles of heeling are not limited, but are considered as being of any magnitude, the requisite mensurations are indeed more troublesome, but are not liable to more errors in execution than in the former case, when the angles are limited to those which are evanescent.

The theorems for measuring the stability of ships, which are founded on assuming the angles of inclination from the position of equilibrium evanescent, explain, in the most satisfactory manner, the principles on which the stability of ships, when heeled to small angles of inclination, is founded; they also ascertain when ships or other bodies float on the water permanently in a given position of equilibrium, or overset. But this can scarcely ever be an object of inquiry in respect of ships, which are always constructed so as to float upright, even before any ballast or lading has been added to them.

Mons. Romme, in his valuable work on naval architecture, intituled *L'Art de la Marine*, published at Paris in the year 1787, informs his readers (p. 106), that the French ship of the line of 74 guns, called Le Scipion, was first fitted for sea at Rochfort in the year 1779. As soon as the ship was floated in deep water, a suspicion arose that she wanted stability; to ascertain this point the guns were run out on one side, and drawn in at the other; in consequence, the ship heeled 13

* Chapman, chap. i. Clairbois *Architecture Navale*, part. ii. sect. i.

inches (probably meaning at the greatest measure on the side of the vessel): by adding the weight of the men brought to the same side, the depth of heeling increased to 24 inches. This being a degree of instability, which was deemed too great to be admitted in a ship of war, the ship was ordered into port, that some remedy might be applied to the defect which had been discovered. M. ROMME proceeds to relate, that a difference of opinion prevailed amongst the engineers respecting the cause of this imperfection in the ship, and the remedies by which it might be corrected. The chief engineer, who was sent from Paris to Rochfort to direct what measures ought to be adopted on this occasion, and for rectifying the like fault in two other ships of war, L'Hercule and Le Pluton, was of opinion, that the stability of the ship Le Scipion would be sufficiently increased by altering the quality and disposition of the ballast. The original ballast of the Scipio had been 84 tons of iron and 100 tons of stone; according to the new arrangement of the chief engineer, the ballast was composed of 198 tons of iron and 122 tons of stone. But as a ship of war does not admit of any alteration in the total displacement or immersed volume, to compensate for the additional weight of ballast, amounting to 136 tons, the quantity of water with which the ship had been supplied was diminished by the weight of 136 tons. This alteration must necessarily have the effect of lowering the centre of gravity of the vessel, and thereby of increasing its stability: but, on trial, this increase was by no means sufficient; the diminution of heeling measured on the vessel's side being only 4 inches. After this and other ineffectual attempts, the defect of stability was at length remedied by applying a bandage or sheathing of light wood to the exterior sides of the vessel, from 1 foot to 4 inches in

thickness, extending throughout the whole length of the water line, and 10 feet beneath it.

This account shews that the theory of stability, restrained to cases in which the angles of inclination, or heeling, are very small, cannot be relied on for ascertaining the requisite stability of ships in the practice of navigation. It must be supposed that the weight and dimensions of every part of this ship were exactly known to the engineers, yet we observe that the instability was not certainly ascertained, but suspected only to exist when the ship was first set afloat in deep water; and after this defect had been discovered by the experiment which has been related, the cause was sought for in vain, and the remedy at length was stumbled upon by accident, rather than adopted from any knowledge of the principles by which the application of it might have been directed.

It seems allowable to suppose, that if rules for ascertaining stability correspondent to any different angles of heeling, similar to those which are demonstrated in page 60, and exemplified in page 115 of this tract, had been applied to the case in question, they would have discovered that an error in the form* given to the sides of the vessel was the principal cause of the defective stability, and would have suggested the remedy accordingly; or rather would have prevented the necessity of having recourse to it, by previously shewing the original defects in the plan of the ship.

The force of stability by which ships, when inclined round

* Mr. ROMME observes, page 108, that the defect of stability in the Scipio was not occasioned by any want of breadth in the principal section of the vessel; for other ships of the same force, *i. e.* Le Magnifique, Le Sceptre, Le Minotaur, L'Intrepide, the breadths of which were the same, or rather less, than that of the Scipio, carried their sail perfectly well.

the longer axis from their position of equilibrium through different angles, endeavour to regain that position, is to be considered in two points of view respecting the motion of a vessel at sea; first, in relation to the resistance by which it opposes any force that may be applied to incline the ship, for instance, that of the wind; in which case the ship's stability, and the impulse of the wind, constitute a species of equilibrium as long as the wind continues of the same intensity. Secondly, the force of stability is to be considered as operating on the ship, after the force by which it has been inclined ceases, to restore the vessel to its upright position; the ship being continually impelled by the force of stability, revolves round an horizontal axis, passing through the centre of gravity with an increasing velocity, till it arrives at its upright position; and afterwards with a velocity continually retarded, till it arrives at the greatest inclination on the other side. This rolling of the ship, with alternate acceleration and retardation of the angular velocity, will evidently depend on the force by which the angular motion is generated; that is, on the force of stability, and its variation corresponding to the several angular distances of the vessel from its upright position; from this cause arises one of the principal difficulties in the practice of naval architecture; *i. e.* to give a vessel a sufficient degree of stability, and at the same time to avoid the inconveniences which proceed from an angular velocity of rolling, increasing and decreasing too rapidly. It is certain that the variation of the force of stability depends principally on the shape given to the sides of the vessel, which admit of being so constructed (all other circumstances permitting) that the force shall increase either slowly or rapidly to its limit.

From the preceding investigations we observe that some float-

ing bodies, during their inclination from 0° to 90°, pass through a position of equilibrium, in which the force of stability becomes evanescent: in other bodies, no limit of this kind takes place; a difference which depends partly on their forms, and partly on the disposition of the centres of gravity of the solids and of the immersed volumes. It may be satisfactory to consider, in a general view, the effects produced on the motion of ships by the different proportions of their stability while they are inclined round the longer axes. If a vessel* should be of a cylindrical form, floating with its axis horizontal, the vertical sections must necessarily be equal circles: supposing the centre of gravity of such a cylinder to be situated out of the axis, the vessel will float permanently with its centre of gravity, and the centre of the section passing through it, in the same vertical line: if such a vessel should be inclined from the upright by external force, it will be impelled in a contrary direction by the force of stability, which increases exactly in the proportion of the sine of the angle of inclination: it is plain, therefore, that a vessel of this description, during its inclination by heeling, cannot arrive at any limit where the force of stability is evanescent; on the contrary, it must continually increase until the inclination is augmented to 90°, where it will have become greater than at any other angle.

Let another case be assumed: suppose the form of the vessel to be a square parallelopiped, floating permanently with one of the flat surfaces upward; when this solid has been inclined round the longer axis through 45 degrees, the stability will be evanescent, and the least inclination greater than that angle

* This is evidently an hypothetical case, stated with a view of illustrating the subject.

will cause the vessel to overset: in this case, as the vessel is gradually inclined from the upright, the stability will first increase to a maximum, and afterwards decrease; differing altogether from the variation of the stability in the preceding case, when the vessel was supposed to be of a cylindrical form. Although vessels are usually so constructed that during any inclination from 0° to 90° they do not pass through a position of equilibrium; yet there seems reason to suppose that in some vessels the stability increases to a maximum, and afterwards decreases when the angle of inclination is farther augmented: whenever a vessel of this description should be inclined beyond the angle where the stability is greatest, the following consequence must necessarily ensue; if the angular velocity should be considerable, the rolling of the ship will be extended to large angles of inclination, because when the stability is more and more diminished as the angle of inclination is augmented, more time will be required for the diminished force to react against the ponderous mass of the vessel, in order to restore it to the upright. It is certain that the angle, as well as the celerity or slowness of rolling, depend on other elements, as well as on the stability, particularly on the weight and extent of the masts and sails, and the position of the ballast and lading: but in comparing the vibrations of the same vessel through different arcs, those elements are the same, while the force of stability alters continually as the angles of inclination are increased or diminished.

These alternate vibrations of a ship in rolling have been deemed analogous to the oscillations of a pendulum; and in order to reduce to some kind of measure so essential a quality of vessels, M. BOUGUER and other writers propose to find a pendulum isochronal to the oscillations of a ship. This pro-

blem seems to imply both that the pendulum sought, and the vessel itself, shall vibrate in arcs that are extremely small; for otherwise the analogy altogether fails: no oscillating body can describe arcs of unequal lengths in equal times, unless it is impelled by forces which are in the direct ratio of the distances from the quiescent point; and therefore the oscillations of a vessel vibrating in different finite angles are evidently not isochronal with each other, since the force of stability varies in a proportion so different from that of the distances from quiescence; nor can they be isochronal with any pendulum, unless the arcs of vibration are of evanescent magnitude; in which case the force of stability being in the direct proportion of the angles of inclination from the upright, has the effect of producing an equality in the times of oscillation: to ascertain a pendulum vibrating in small arcs which is isochronal to the oscillations of a vessel, under these restrictions, is a problem which may be solved with sufficient exactness; but unless the limitation that has been mentioned should be specified, it is a question without the necessary conditions. Mons. BOUGUER* in his chapter intituled, *que les Oscillations sont Isochrones*, does not expressly mention this limitation, but we must allow it probable that he conceived it to be implied.

From the reasons that have been stated it seems to follow, that in order to form a satisfactory opinion of the qualities and performance of a vessel at sea as depending on the plan of its construction, the forces of stability at the several angles of inclination from o to the greatest limit ought to be ascertained, particularly the measure of the greatest stability, and the angle of heeling at which it takes place.

* Liv. i. sect. iii. chap. vii.

In these general remarks the water's resistance has not been considered, which must necessarily have some effect in retarding the oscillations of the vessel, and more in the larger arcs than in the smaller: it is however observable, that the resistance to the rolling of vessels is of a very different kind to that which is opposed to their progress through the water, in which case a volume of the fluid proportional to the vessel's bulk and velocity is entirely displaced during its motion; whereas in the rolling of ships a far less quantity of water suffers an alteration of place by the ship's oscillations, which is therefore the less retarded on this account.

Another observation occurs on this subject. The entire stability of a ship has been shewn to consist of the aggregate stabilities of the several vertical sections into which it can be divided. Let it be supposed that the ship has been inclined round the longer axis through a given angle, and that the vessel returns through the same angle of inclination by the force of its stability; if the forces arising from the several sections do not act in their due proportion on each side of the centre of gravity, in respect to the longer axis, the ship will not return to its position of equilibrium by revolving round the longer axis; but will be inclined round various successive horizontal lines between the longer and shorter axes; a circumstance that must create irregular motions and impulses, to which a vessel in all respects well constructed is not liable.

The theory of statics and mechanics was, I believe, first applied to explain the construction and management of vessels toward the latter end of the last century, in a work intituled *Théorie de la Construction des Vaisseaux, par* P. PAUL HOSTE, printed at Lyons in the year 1696. Several eminent mathematicians have since prosecuted this difficult subject,

particularly JOHN BERNOUILLI, BOUGUER, and the excellent M. EULER, whose treatise, intituled *Théorie complette de la Construction & Manœuvre des Vaisseaux*, is a work correspondent to the title, entirely theoretical. In this elaborate performance the author has not only endeavoured to explain the complicated laws which influence the motion of ships at sea, but proceeds to investigate, on the ground of such data as the subject affords, the dimensions and position of the most essential parts of vessels which combine to give them every possible advantage in the practice of navigation.

Several inquiries are suggested by the perusal of these theoretical works; first, whether the proportions and dispositions of parts in ships resulting from theory have been found to differ from, or to agree with, those which had been previously established in the practice of naval architecture; secondly, if disagreement should have been discovered, whether any adequate and satisfactory trials have been made to ascertain the advantages which result from adhering to the constructions prescribed by practice, compared with those which are consequences of following the deductions from theory; and lastly, if any new forms of vessels, disposition of parts, or other varieties of construction, have been discovered by considering this subject in a theoretical view, and in what degree these inventions have been found advantageous when applied in practice.

Exclusive of the application of geometrical principles,* by

* Practical treatises on ship-building have been published by various authors, particularly by M. CLAIRBOIS, ROMME, and FRED. CHAPMAN. In these useful works theory is occasionally applied to explain and illustrate the principles of naval architecture: but no accounts are to be found in either of these volumes, as far as my researches extend, by which the construction of vessels, founded on theoretic investi-

which the forms of vessels and the disposition of their most essential parts are ascertained, theory may be considered as bearing to naval architecture a two-fold relation: first, as depending on the pure laws of mechanics, a subject on which the preceding cursory observations have been offered: secondly, the practice of naval architecture is guided, in most parts of the world, by a species of theory or systematic rule which individuals form to themselves from experience and observation alone: it is founded on the experimental knowledge in naval constructions, which has been transmitted from preceding times, combined with the more recent improvements, and includes whatever inventions of skill and ingenuity are applicable to the various machinery that is employed in the construction and management of vessels: by repeated observation on the forms, proportions, and equipment of ships, and by attention to their excellencies and defects when afloat at sea, faults are remedied, good qualities are improved, and rules of practice are by degrees established according to principles, well perceived and understood, without much assistance from the theories of mechanics, statics, and geometry, on which such principles are founded: for in this, as well as other instances, it is well known that skilful practice, aided by long experience, arrives at determinations which it is very difficult (sometimes impossible) for theory to infer: on the other hand it must be allowed, that pure theory, depending on the laws of motion, the subject of disquisition in

gation, have been subjected to practical examination during voyages. M. CHAPMAN, in page 79 of his work (Paris edit.), expresses the proportions and disposition of parts in vessels by algebraic quantities, which, however, are not to be mistaken for deductions from theory; since the author has not pointed out any mode of investigation, or train of reasoning, by which those expressions can be deduced from the principles of mechanics.

the works of M. EULER and BOUGUER, is of great importance to the advancement of this science:—for by such investigation, so far as the data are sufficient, the qualities of vessels are traced to their true causes, and are explained by general laws; whereas the principles derived from mere observation are scarcely ever applicable beyond the cases in which they have been experienced in practice.

Whatever may have been the means by which naval architecture receives progressive improvement, it seems to be generally allowed, that the art of constructing vessels has, at the present period, attained to a degree of perfection far surpassing any that has been known to former times, either ancient or modern; yet it is equally certain, that some principles, by which the construction of vessels is materially influenced, still remain to be developed and explained. It is frequently remarked by navigators, as well as by naval architects, that alterations apparently the most trivial, in the form of a vessel, in the distribution of the ballast, or in the position and extent of the masts and sails, will wholly change the qualities of a ship from bad to good, or the reverse. As these changes cannot be attributed to fortuitous causes, it is necessary to allow that they are consequences of principles certain and definite, though in many cases unknown, or imperfectly estimated by conjecture. The proportions and disposition of parts, which operate to produce good or bad effects on the sailing of ships, are probably in these instances so intricately combined as to make it scarcely possible from mere observation, however extended and diversified, to account satisfactorily for changes so remarkable: it must also be acknowledged, that some of the data on which the theory of naval architecture is founded, being imperfectly known, parti-

cularly the laws of the different resistances to the ship's motion,* it would be unsafe to rely entirely on deductions *a priori* for explaining this subject.

* The laws of resistances, opposed to bodies which move in fluids, and varying in a duplicate ratio of the body's velocities, are demonstrated by Sir ISAAC NEWTON, in the second book of the *Principia*, on conditions restrained to the particular case in which the motion of the resisted body is extremely slow, and the fluid perfectly compressed. On these conditions, the pressure which resists the motion of the body is exactly balanced by the pressure on the posterior part, and consequently the only force opposed to the body's motion, is the *inertia* of the fluid, which is displaced while the body moves through it: for the resistance of friction depending on the body's velocity must be, in a physical sense, evanescent, when the motion is very slow. It is evident, that the theory of resistances founded on these principles ought not to be applied to the solution of cases in which the velocity is much increased, without great care and circumspection; for by the increase of velocity, three different forces begin to have operation, of which the NEWTONIAN theory takes no account; *i. e.* the pressure on the anterior part of the body, the pressure on the posterior part, and the resistance of friction. The pressure on the anterior part will evidently be a constant or invariable quantity as long as the moving body continues at the same depth. The pressure on the posterior part will depend on the velocity of the body's motion, and when that velocity is $= 0$, the pressure will be precisely equal, and contrary to that which acts on the anterior part. Moreover, when the body's velocity is equal to that with which the fluid rushes into empty space, the pressure on the posterior part will be $= 0$, and of consequence all the pressures on the posterior surface, corresponding to the intermediate velocities, must be found between these limits. When the surfaces of the moving body are smooth, it has been supposed that the effects of friction are not very considerable. This opinion is however disproved, to the satisfaction of any one who consults the account of the very accurate and well devised experiments on the motion of bodies through the water, made under the direction of the committee of the Society for the Improvement of Naval Architecture, and published by their order. I have examined these experiments with a good deal of attention, particularly those which were made on oblong beams or parallelopipeds, denoted in the account of the experiments by the letters A, B, &c.; and find, that although the surfaces of the moving body were planed very smooth, the resistance of friction was equal to a weight of no less than ninety pounds, on a surface of 258 square feet, when the body moved with a velocity of 8 feet in a second. It appears also, by methods of calculation, founded on Sir ISAAC NEWTON's rule for drawing a parabolic line through any number of given points situate in the same plane, and applied to the above-named experiments, that the resistance of friction varies in no power of the velocity expressible by less than three di-

These difficulties will appear still greater, if it be considered that the causes which influence the motion of ships at sea are not separate and independent, but operate on each other, as well as immediately on the motion of the vessel: thus, if the position of the centre of gravity is altered by moving the ballast or lading nearer to the head or stern, this alteration will have the effect of changing the section of the water, and the form of the immersed part of the vessel; on which account, the resistance opposed by the water to the ship's motion must necessarily be changed; the centre of gravity of the part immersed will also be differently situated, which must combine

mensions thereof, that is, if z is put to denote the resistance of friction, and v to denote the velocity, the resistance requires an equation of the form $z = au + bu^2 + cu^3$; in which a, b, and c, are invariable quantities: the force also of pressure on the posterior surface is expressed by an equation equally complex: to these difficulties another is to be added, which is, that the resistance varies with the depth of the moving body, as appears by the experiments referred to. On these considerations it seems manifest, that investigations on the subject of naval architecture, founded on the theory of motion, which takes into account the resistances of the water, considering the velocity to be such as ships usually sail with, must involve algebraic expressions so complicated, as to make it very difficult, perhaps impossible, to infer any useful practical conclusions from this mode of considering the subject. EULER and BOUGUER, the principal authors who have attempted to apply the theory of resistances to naval architecture, suppose the resistance to be in a duplicate ratio of the velocities; a law evidently different from that according to which vessels at sea are opposed by the medium in which they move: and one of these most eminent authors,* doubts whether this theory is not too imperfect to be relied on, when it is applied to ascertain the motion of ships at sea. Notwithstanding the impediments which arise from the complicated laws of resistance and friction, the general principles investigated in the works of these authors are no doubt capable of being applied to the solution of many difficulties which occur in considering the subject of naval architecture, due allowance being made for those irregular forces which cannot be included in the theoretic solutions.

* EULER. *Théorie complette de la Construction des Vaisseaux*, English edition, p. 93, 94.

with the alteration of the centre of gravity of the vessel, and the section of the water, to increase or diminish the stability of the ship; and it must be added, that the inclination of the masts and sails to the horizon, and the direction in which the wind impinges on them, will suffer alteration from the same cause.

Although theory alone may not be adequate to the solution of these difficulties, yet, when combined with experiments and obervations, it may be probably employed with great advantage in these researches. If the proportions and dimensions adopted in the construction of individual vessels are obtained by exact geometrical mensurations, and calculations founded on them, and observations are made on the performance of these vessels at sea; experiments of this kind, sufficiently diversified and extended, seem to be the proper grounds on which theory may be effectually applied in developing and reducing to system those intricate, subtile, and hitherto unperceived causes, which contribute to impart the greatest degree of excellence to vessels of every species and description. Since naval architecture is reckoned amongst the practical branches of science, every voyage may be considered as an experiment, or rather as a series of experiments, from which useful truths are to be inferred towards perfecting the art of constructing vessels: but inferences of this kind, consistently with the preceding remark, cannot well be obtained, except by acquiring a perfect knowledge of all the proportions and dimensions of each part of the ship; and secondly, by making and recording sufficiently numerous observations on the qualities of the vessel, in all the varieties of situation to which a ship is usually liable in the practice of navigation.

E
IV.
I
D
K

Fig. V.

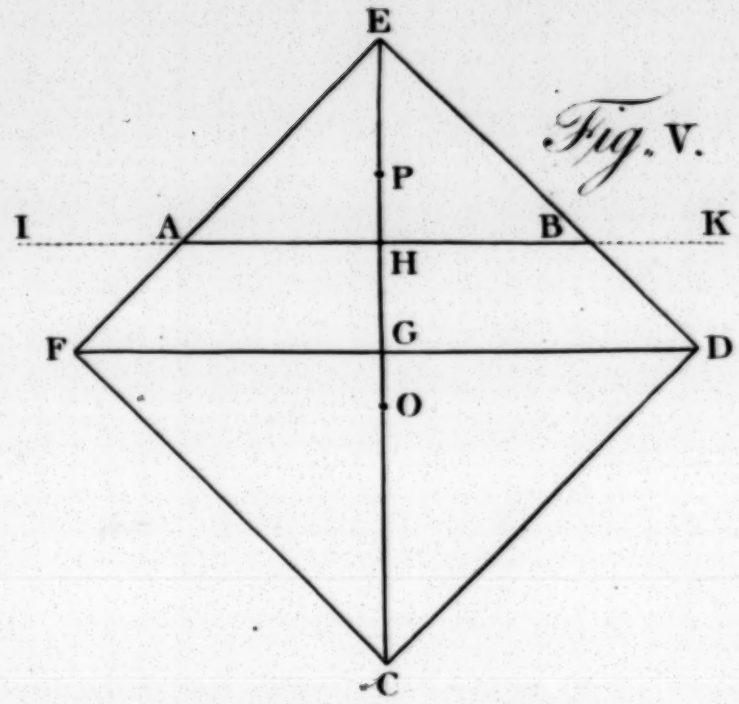


Fig. VI.

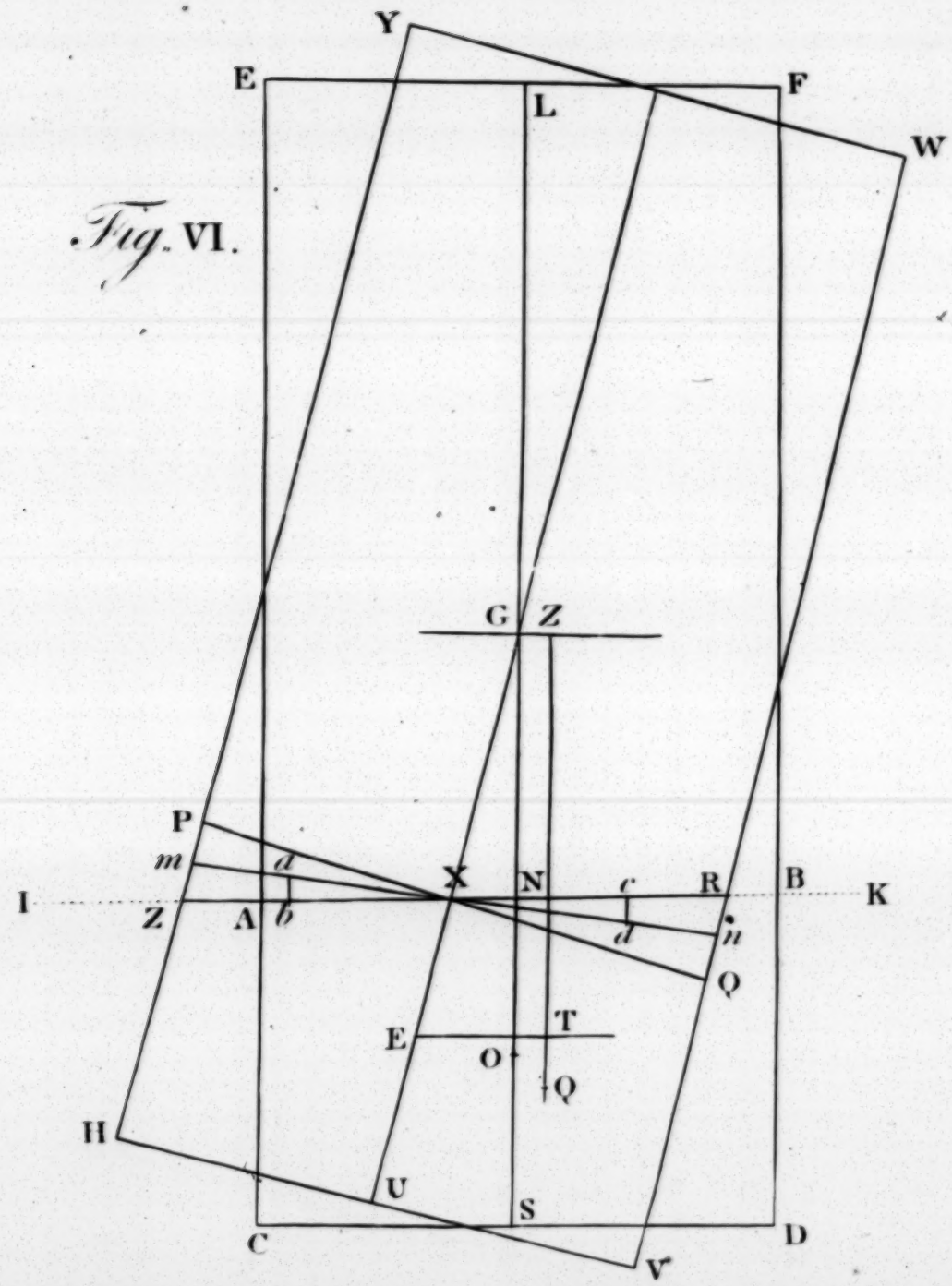


Fig. III.

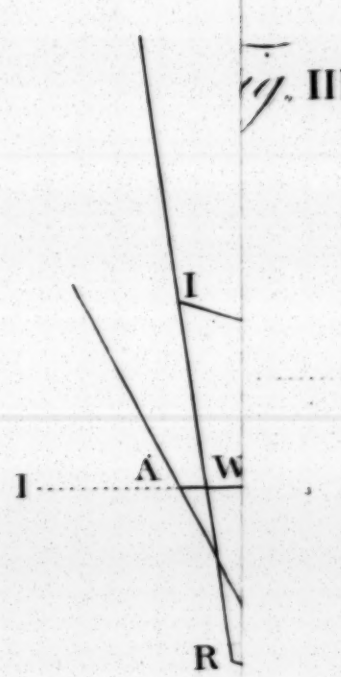


Fig. 1.

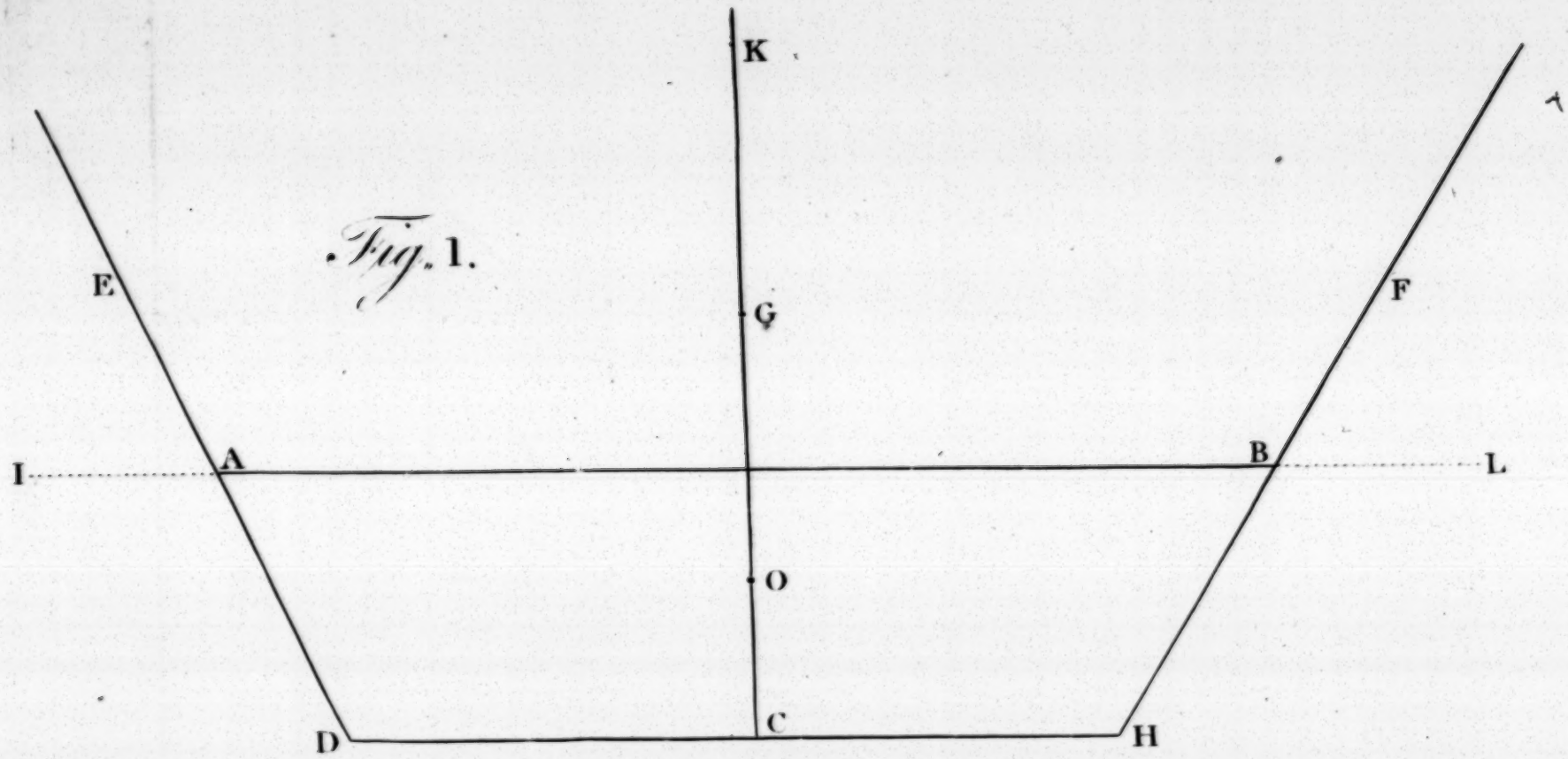
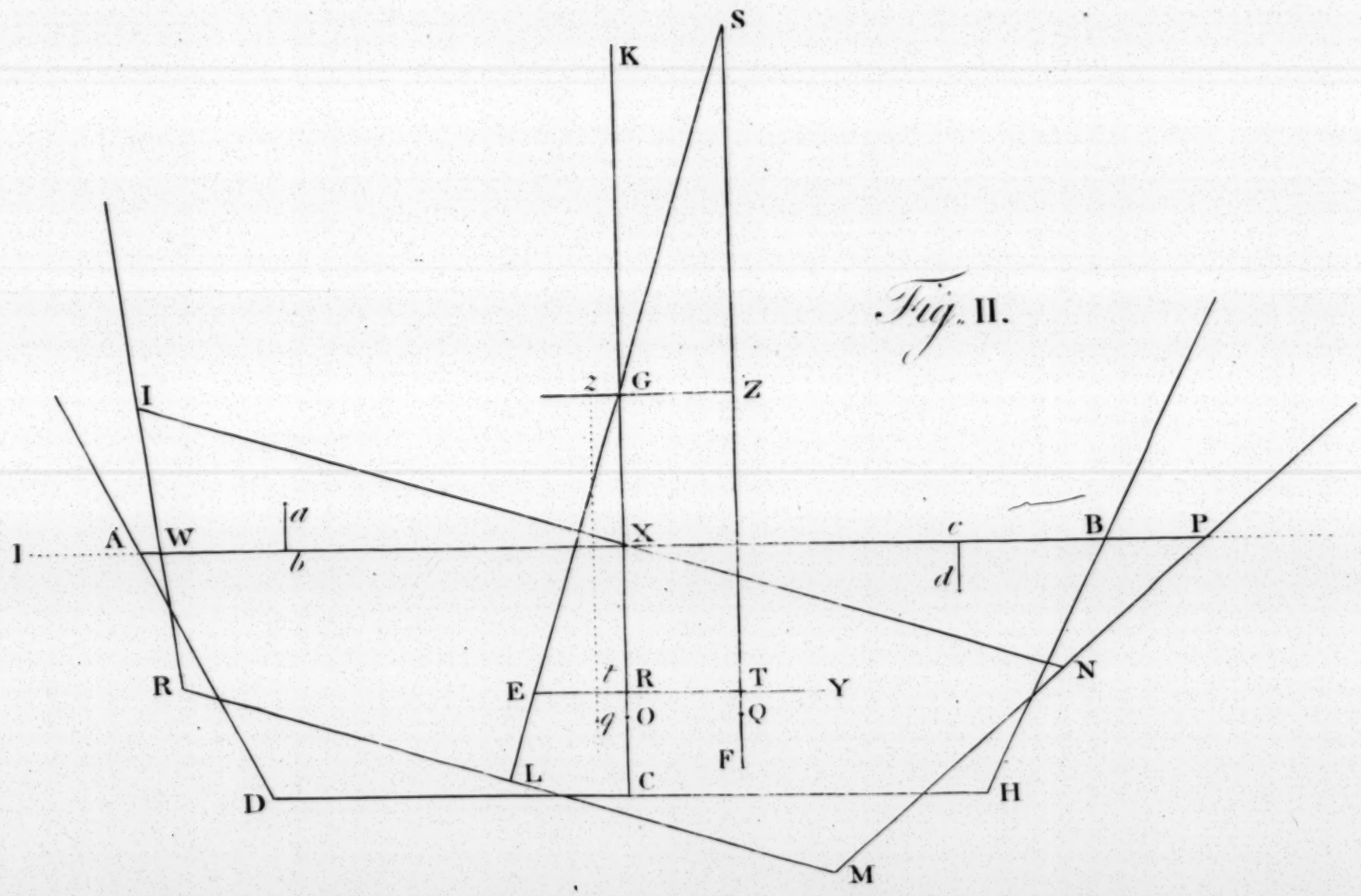


Fig. II.



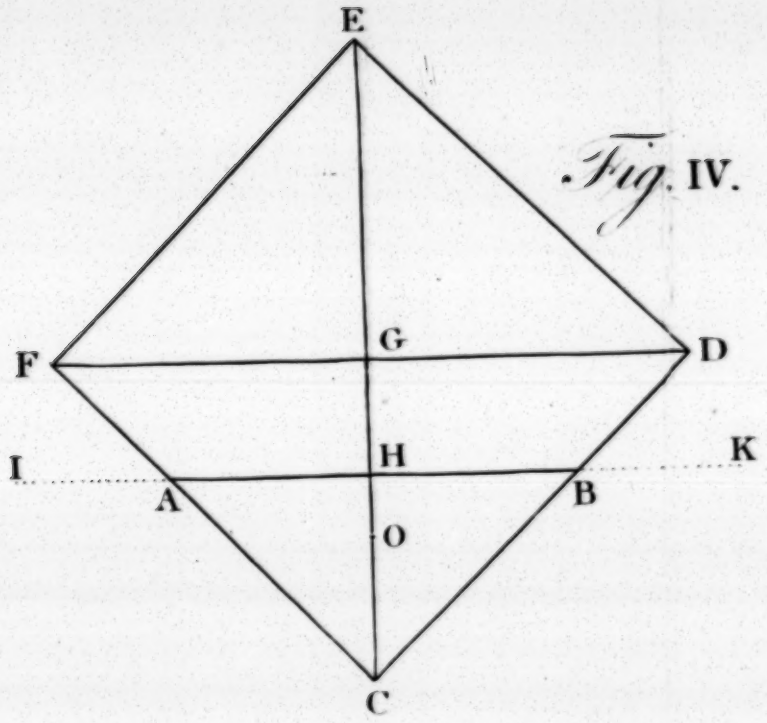


Fig. IV.

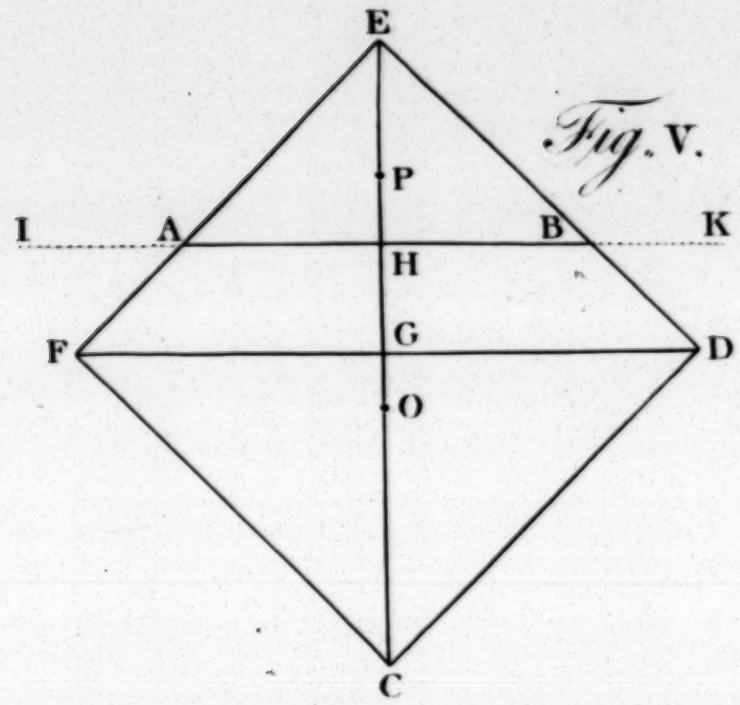


Fig. V.

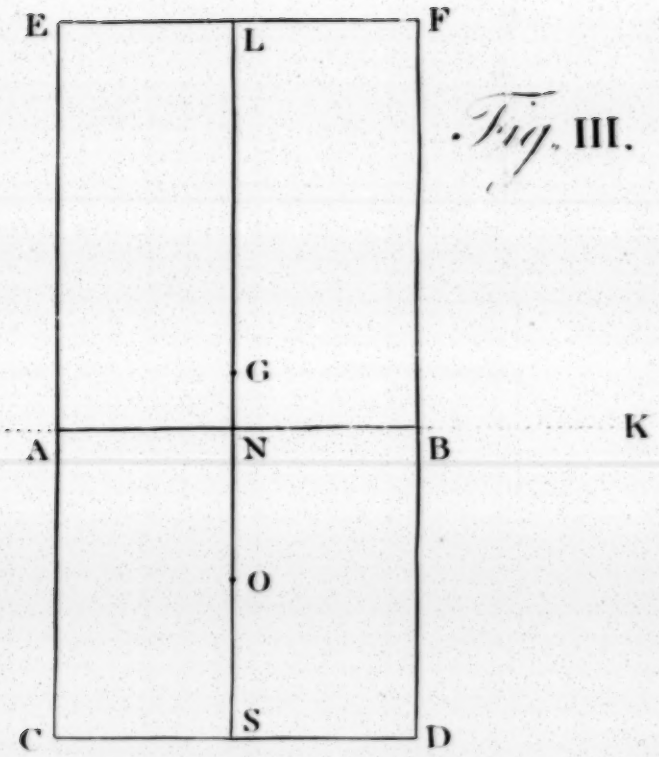


Fig. III.

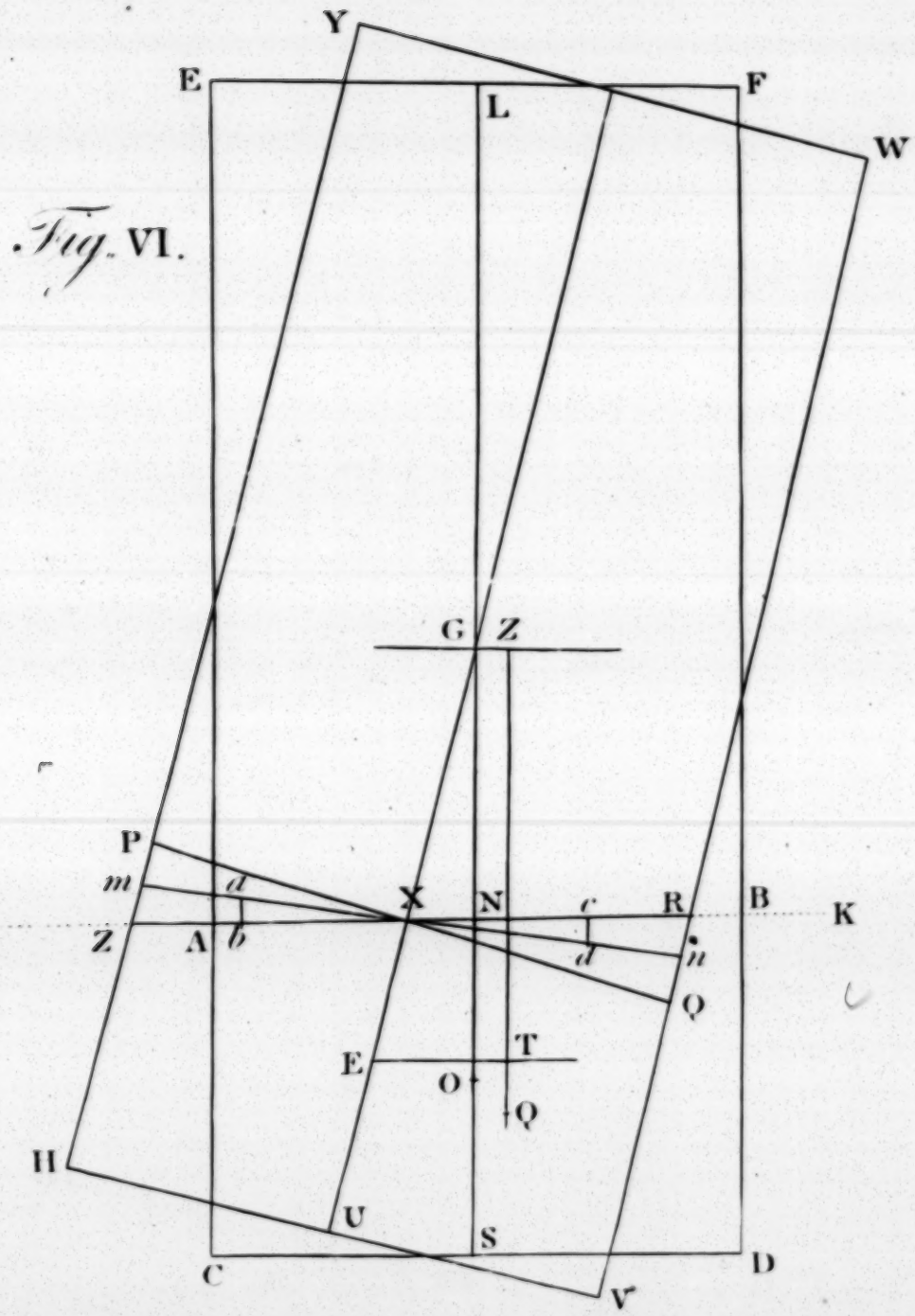


Fig. VI.

Fig. VII.

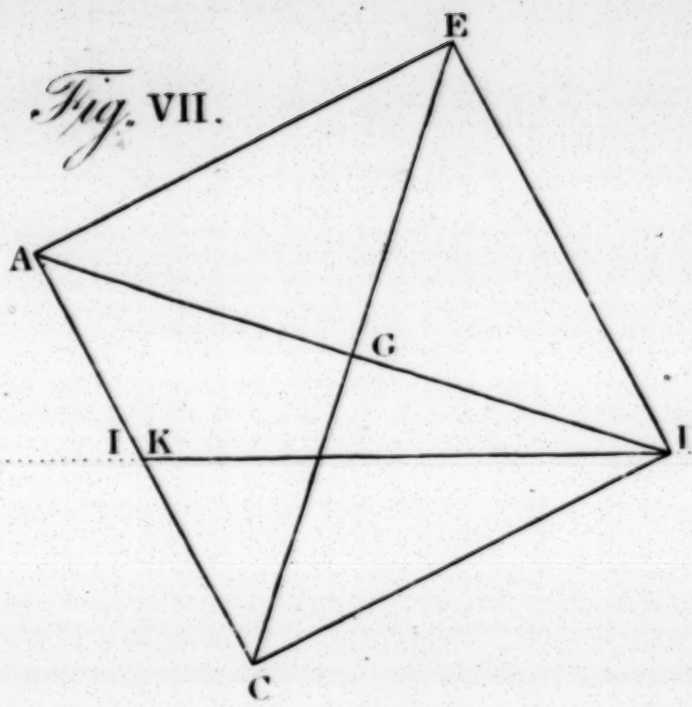


Fig. VIII.

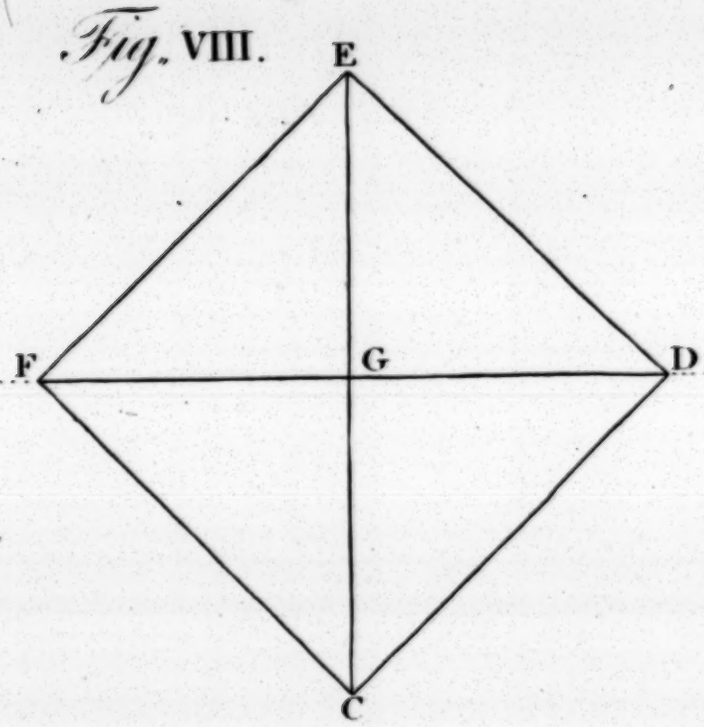


Fig. IX.

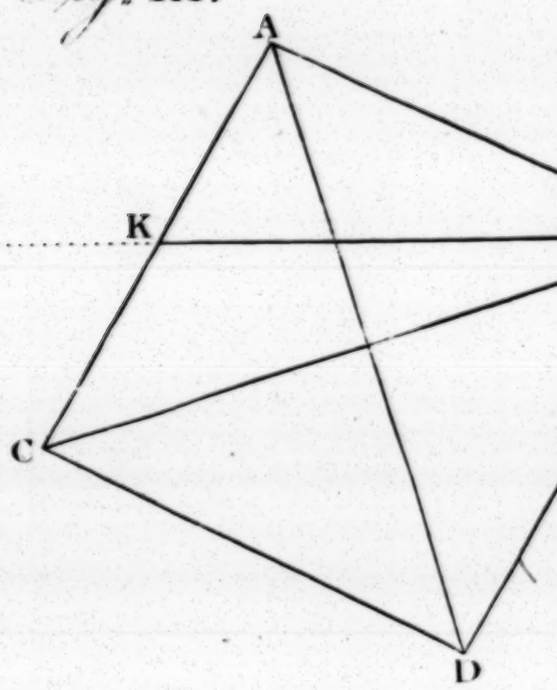


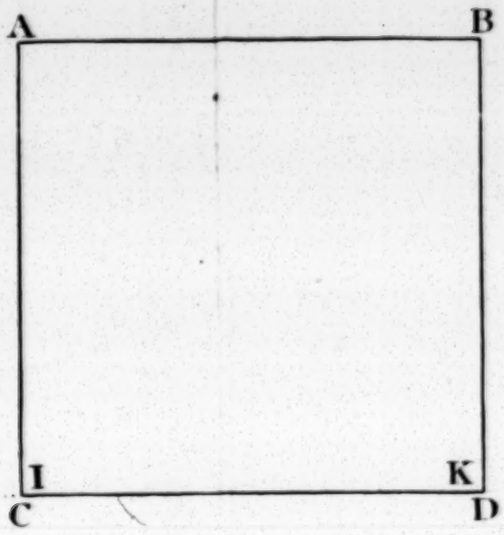
Fig. XII.

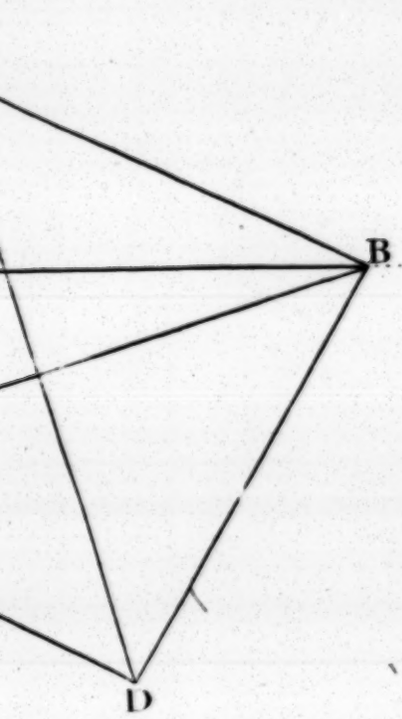
Fig. XIII.

Fig. XIV.



Fig. XI.





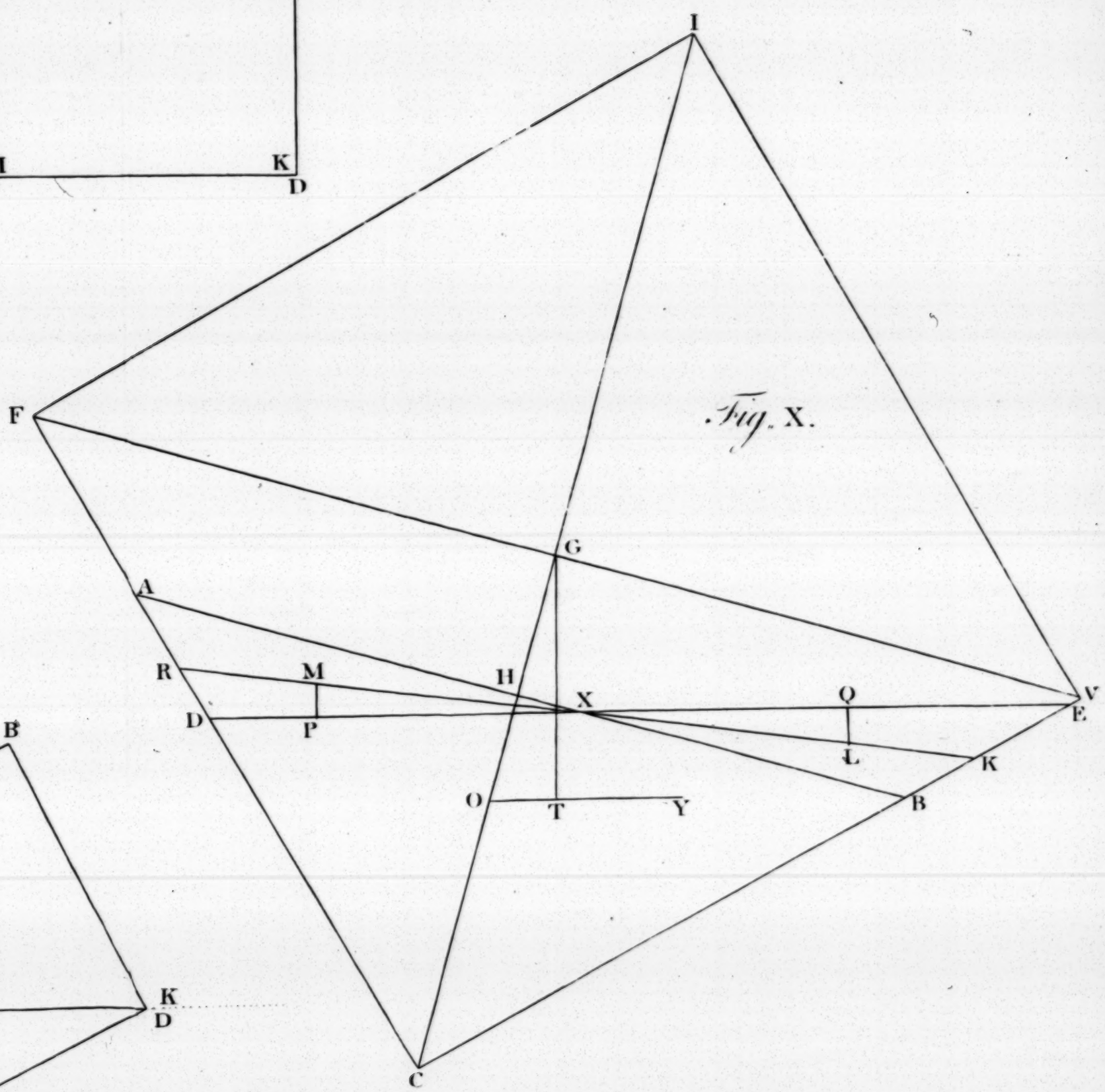


Fig. X.

Fig. XV.

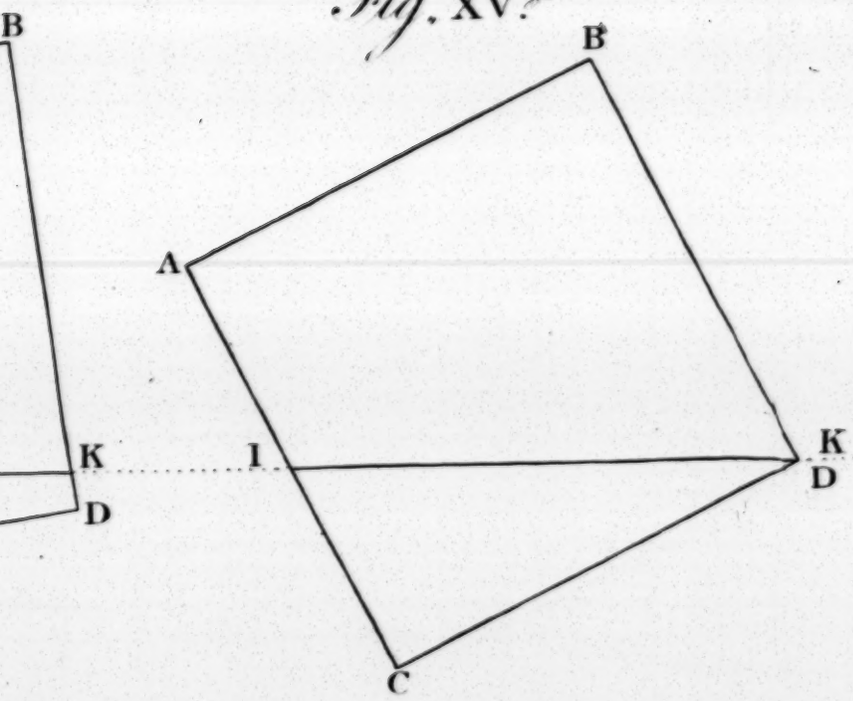


Fig. XVI.

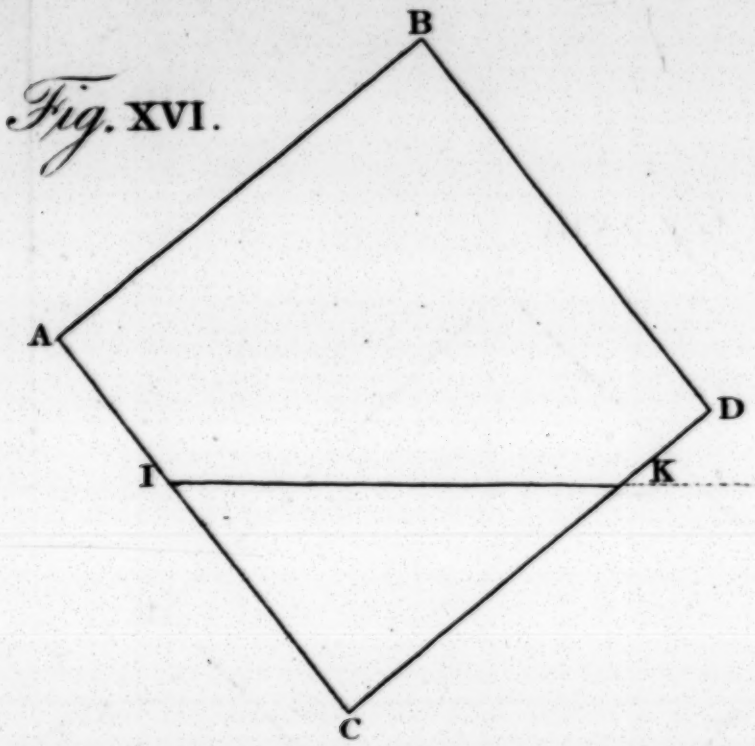


Fig. XVII.

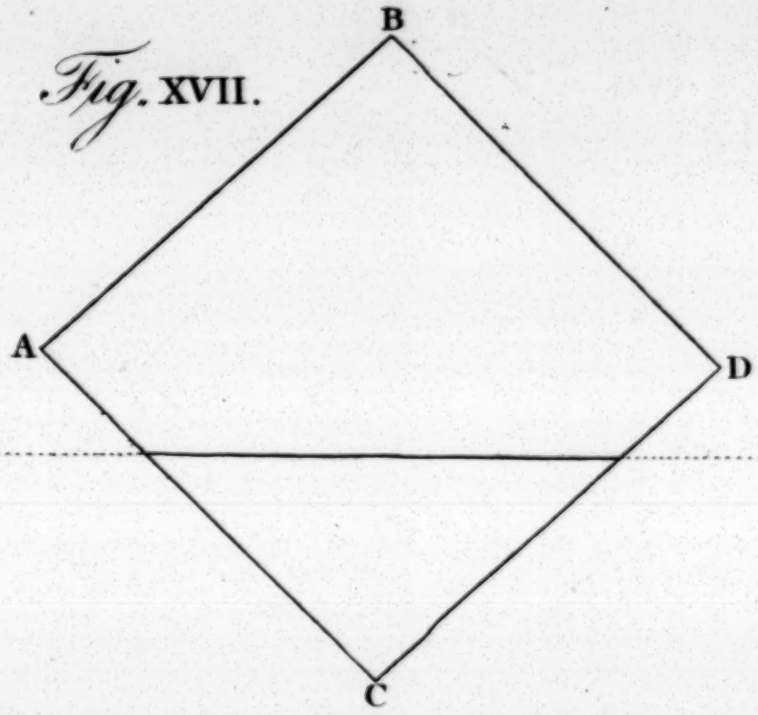


Fig. XVIII

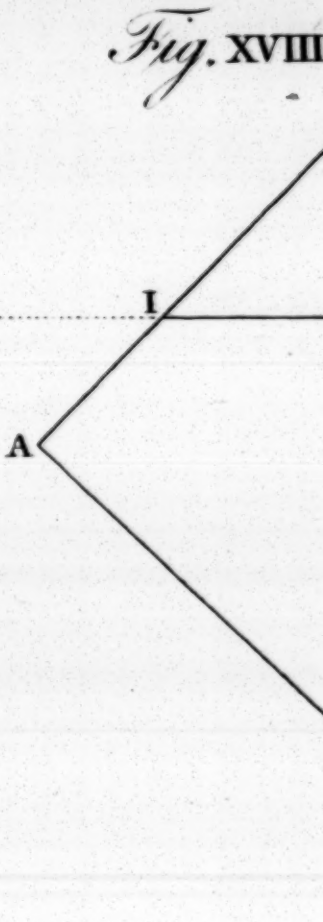


Fig. XXI.

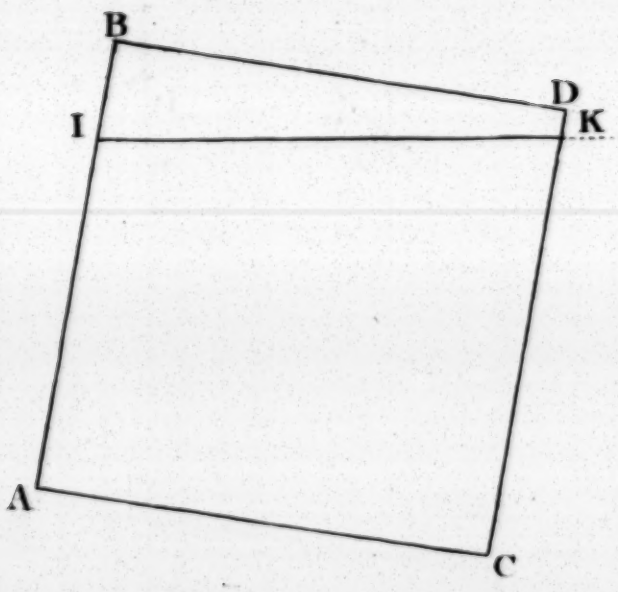


Fig. XXII.

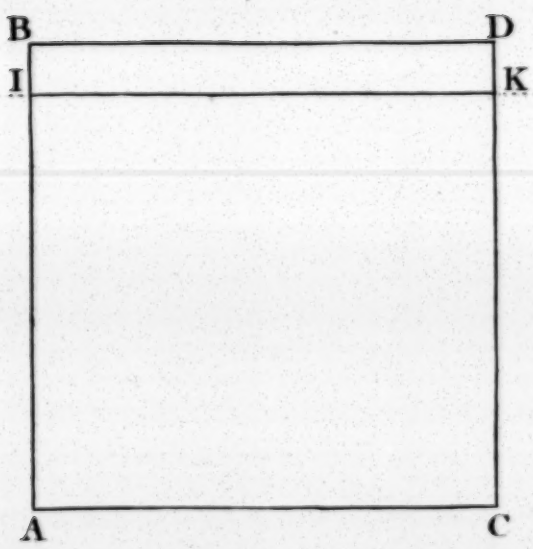




Fig. XVIII.

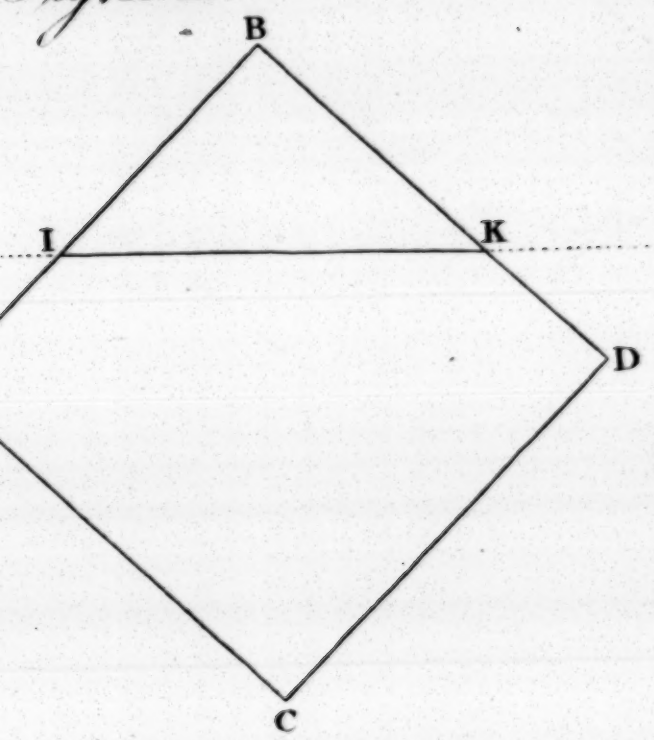


Fig. XIX.

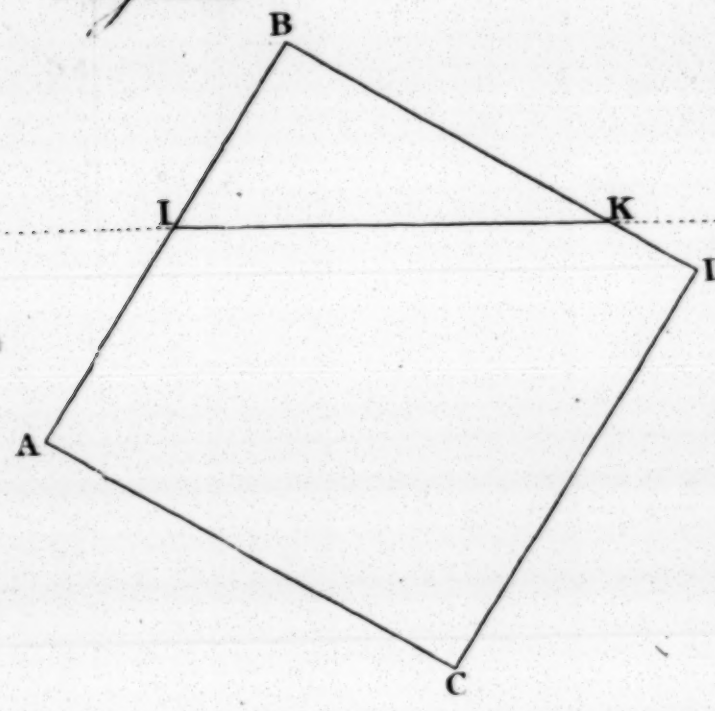


Fig. XX.

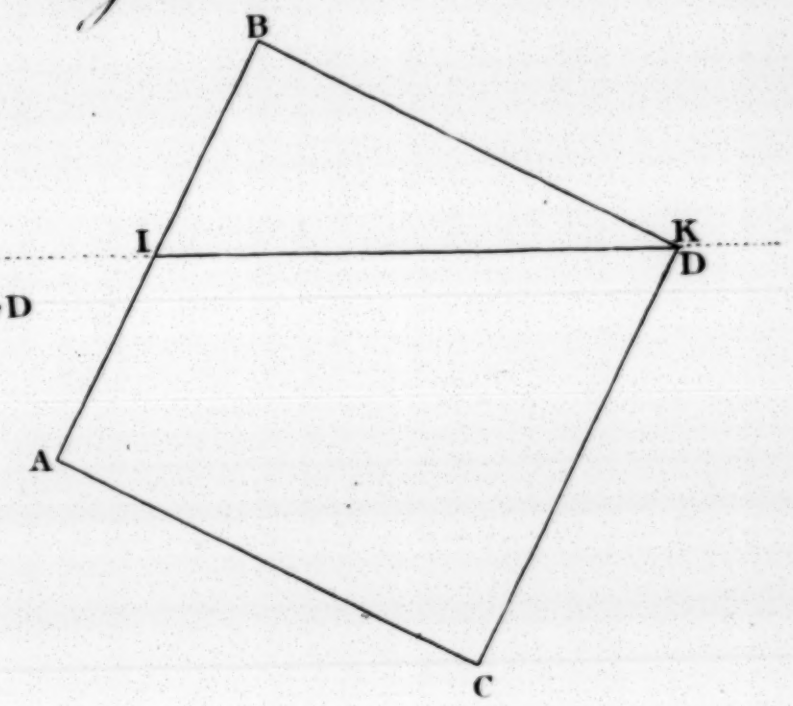


Fig. XXIII.

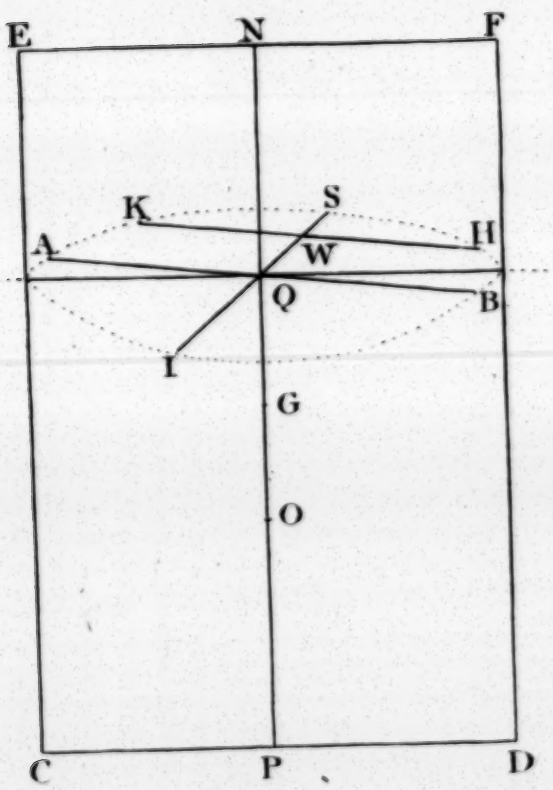


Fig. XXIV.

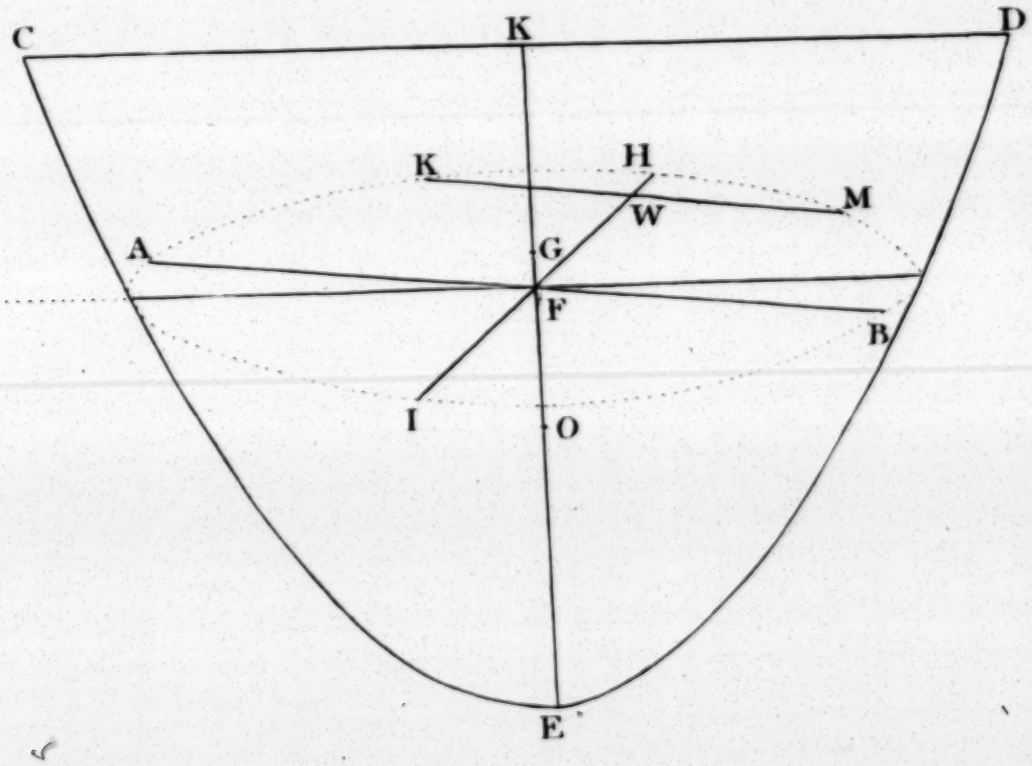


Fig. XXV.

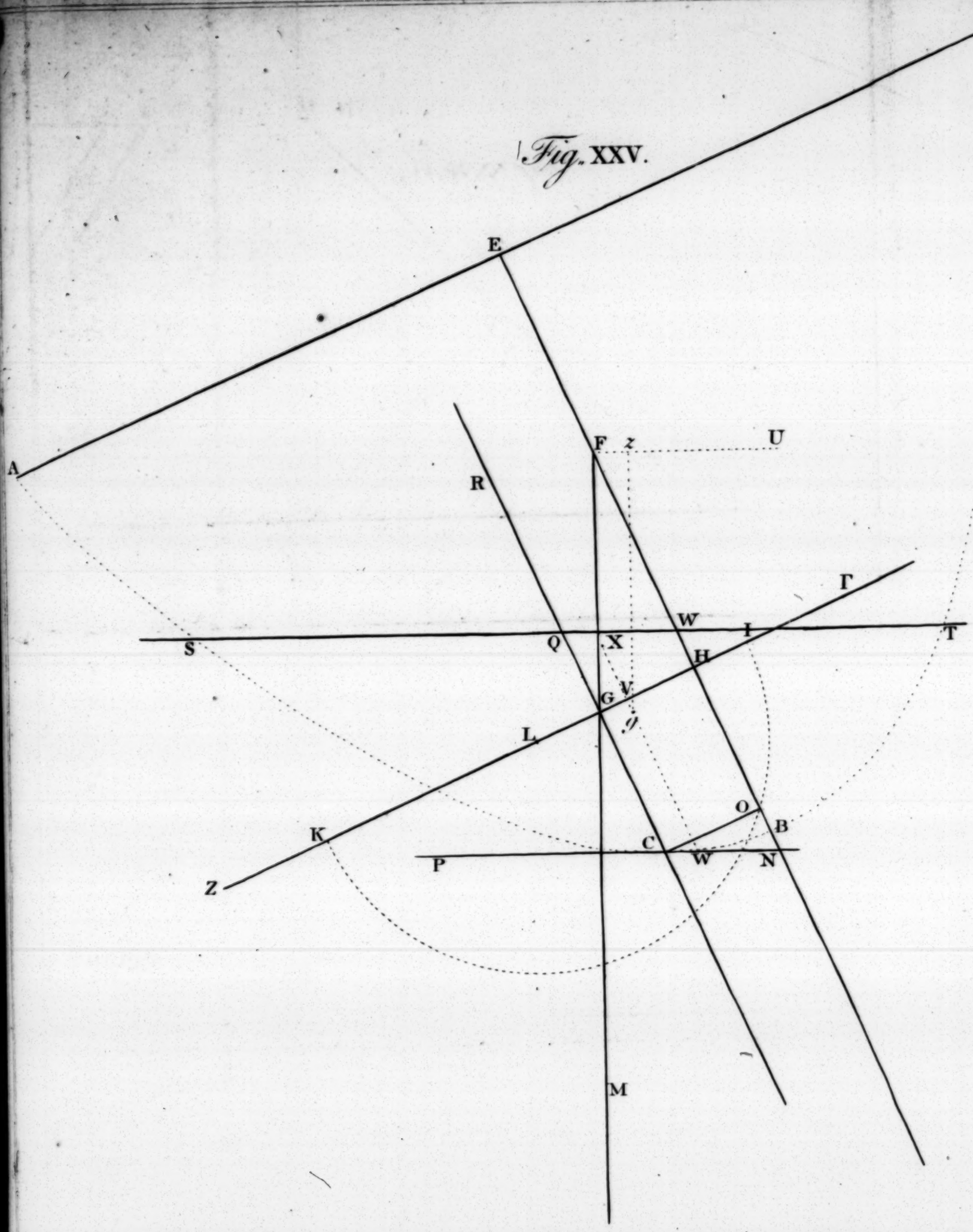
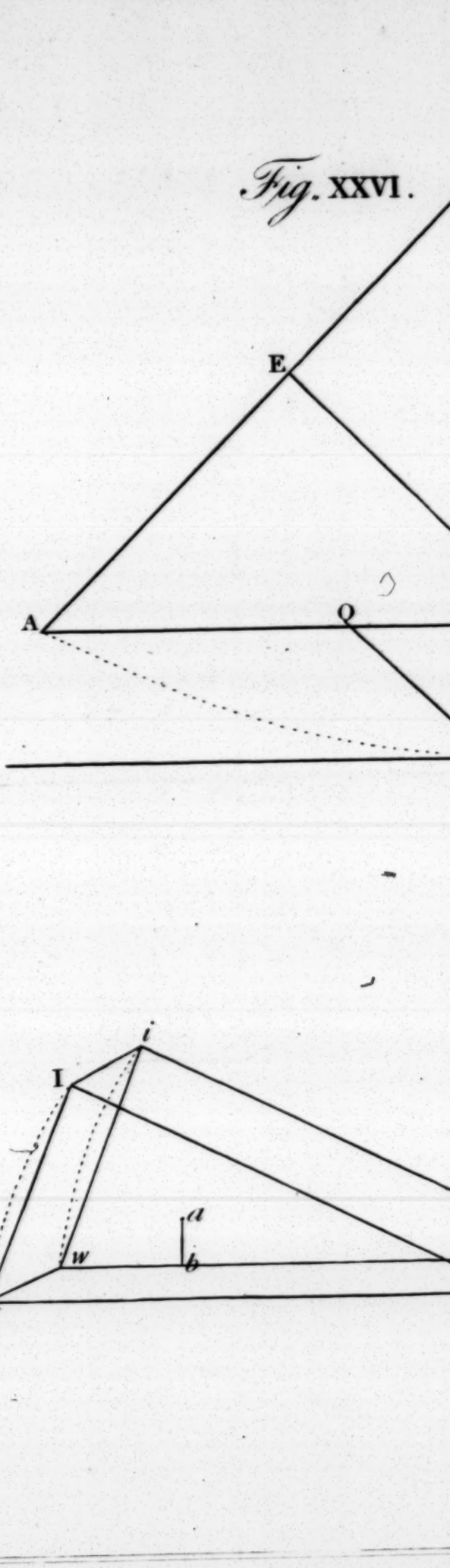


Fig. XXVI.



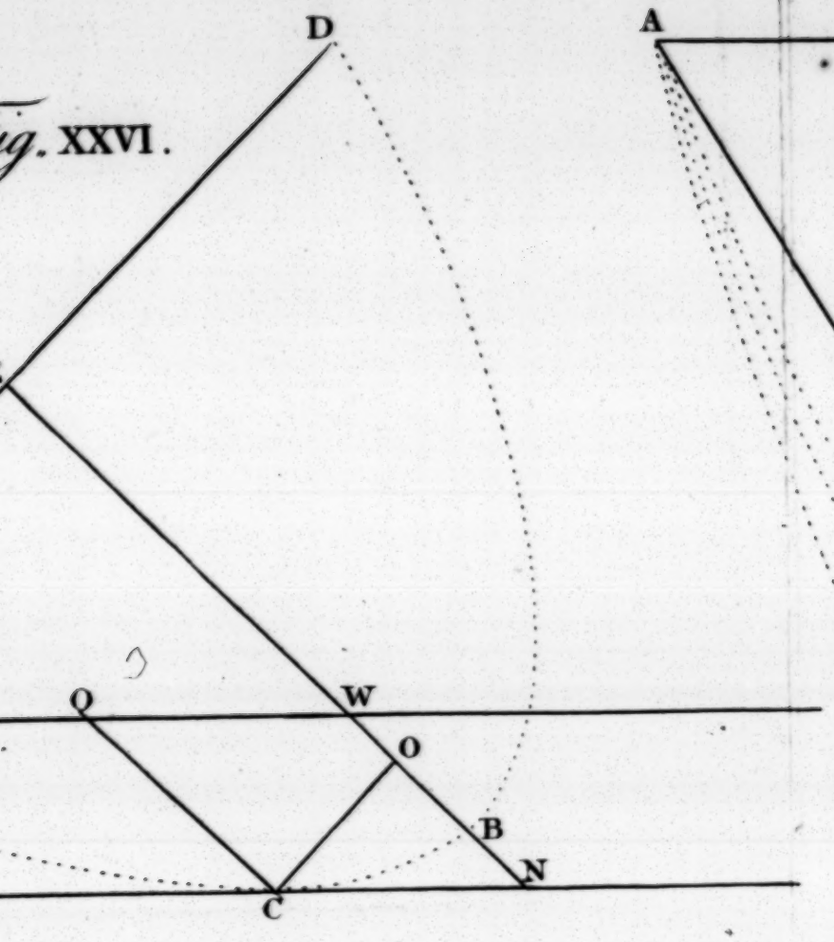


Fig. XXVI.

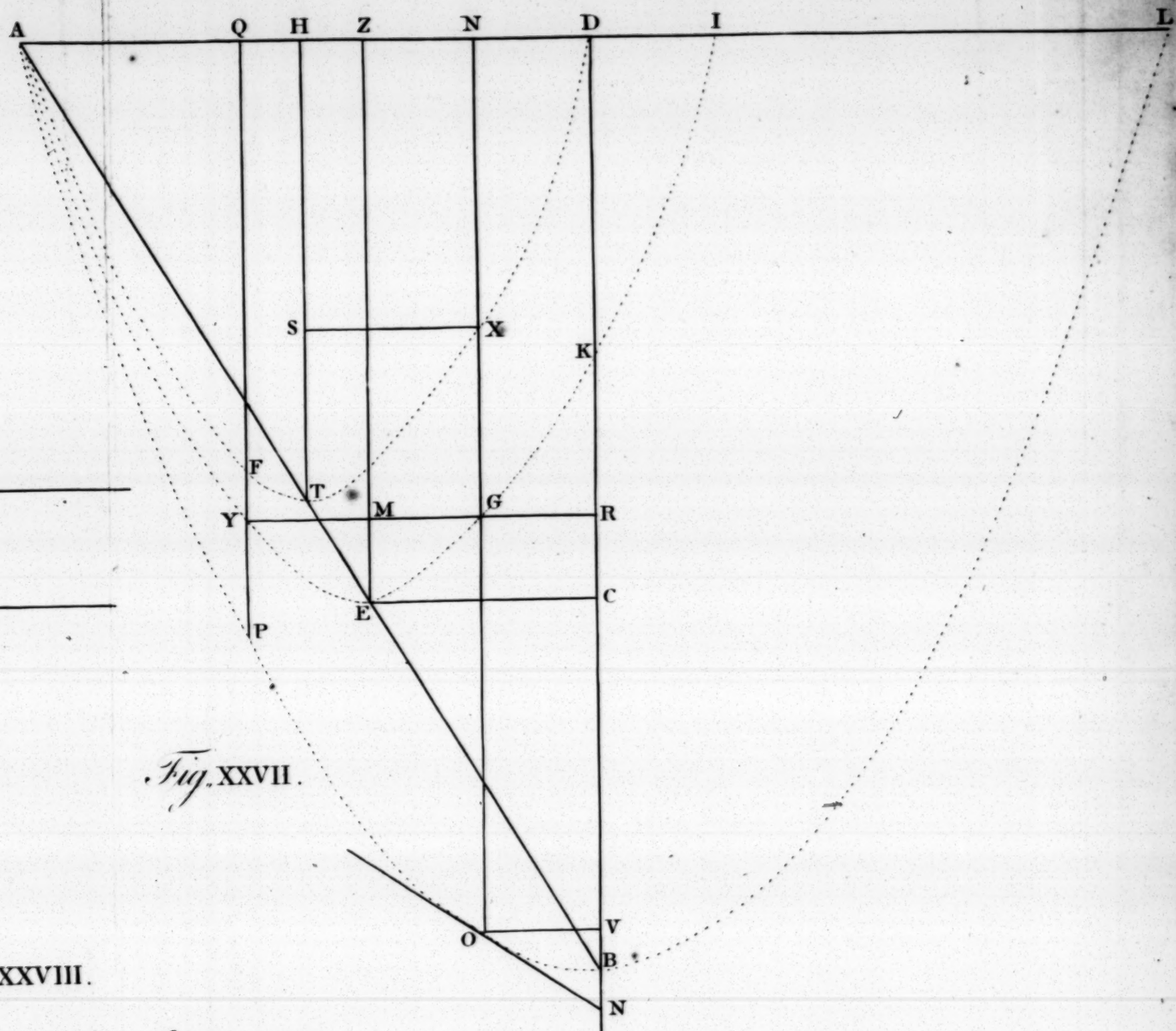
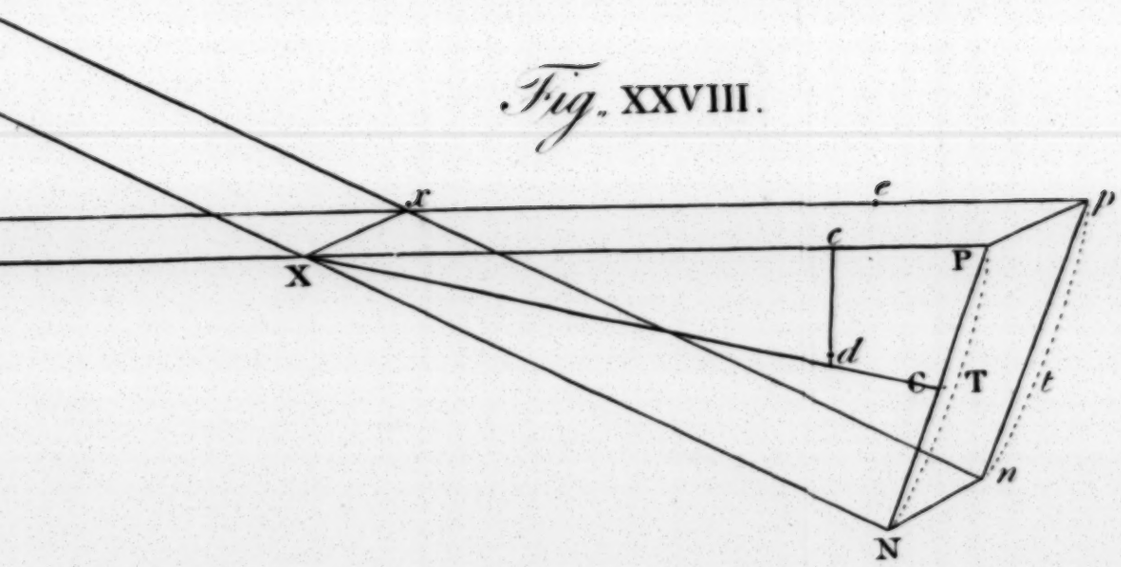


Fig. XXVII.

Fig. XXVIII.



VI. *Account of the Discovery of a new Comet.* *By Miss* Caroline Herschel. *In a Letter to Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read November 12, 1795.

SIR, Slough, November 8, 1795.

LAST night, in sweeping over a part of the heavens with my 5-feet reflector, I met with a telescopic comet. To point out its situation I transcribe my Brother's observations upon it from his Journal.

November 7, 1795.

0^h 33′ Sidereal time. Place of the comet 2° 20′ *np.* 37 (γ) Cygni, in a line continued from 66 (υ) through γ nearly. It is just visible to the naked eye.

0^h 44′. It is in a line between two small stars at a considerable distance from each other, to which are perpendicular the two extreme stars of three other stars, that form a small arch approaching to a straight line.

0^h 49′. The comet precedes the point in the line where the perpendicular of the arch crosses the line of the two stars one-fifth of the distance of the bisecting point from the preceding star.

1^h 25′. The comet is visibly moved from the place where it was 0^h 49′.

The direction of its motion seems to be towards the south preceding side, and it is about 5 or 6′ removed from its former place.

1^h 51′. The diameter of the comet is about 5′. It has no kind of nucleus, and has the appearance of an ill-defined haziness, which is rather strongest about the middle.

2^h 16′. The comet is about 2° 38′ *np* γ, in a line continued from 69 through 37 Cygni.

3^h 37′. The comet is about 2° 50′ *np* γ, in a line continued from 65 through 37 Cygni, or, perhaps more accurate, in a line from 70 continued through 37 Cygni.

It will probably pass between the head of the Swan and the constellation of the Lyre, in its descent towards the sun.

The direction of its motion is retrograde.

Place of the comet deduced from the above.

Nov. 7.	h 0	′ 33	RA	h 20	′ 3	″ 48	PD	° 49	′ 17	″ 18
	3	37		20	0	58		49	37	18

As the appearance of one of these objects is almost become a novelty, I flatter myself that this intelligence will not be uninteresting to astronomers.

I have the honour to be, &c.

CAROLINA HERSCHEL.

Additional Observations on the Comet. *By* William Herschel, *LL.D. F.R.S.*

November 8, 1795.

0^h 10′. The comet is about 42′ north of 22 Cygni, in a line continued from 21 (η) through 22 nearly; it is not quite come to the line.

It is exactly in a line with 22 and a north following star 1° 34′ from 22 towards 21.

0^h 31′. Distance of the comet from 19 Cygni, 1° 10′. From 22 Cygni, about 42′. From 25 Cygni, 2° 10′. From 15 Cygni, exactly 3°.

2^h 27′. The comet is 36′ from 22 Cygni; its motion has been very nearly in the line pointed out before. It will however not pass over 22, but go by it towards 19 Cygni, having left the line pointed out, a little on the following side.

November 9, 1795.

20^h 45′. The comet is about 17 or 18′ from 15 Cygni.

21^h 59′. The comet is now centrally upon a small star north following 15 Cygni. It is a small telescopic star of about the 11th or 12th magnitude, and is double, very unequal, the smallest of the two being much smaller than the largest.

With a power of 287 I can see the smallest of the two stars perfectly well; this shews how little density there is in the comet, which is evidently nothing but what may be called a collection of vapours.

Angle of the small north following star with respect to 15 Cygni, 71° 55′ north following.

Position of the small star belonging to the double star, a few degrees south following.

The north following star must be of the 11th or 12th magnitude at least, for it is not visible in my achromatic finder, and its smaller companion therefore is an extremely small star indeed.

The double star is about 5 or 6′ from 15 Cygni.

November 10, 1795.

21^h 55′. The comet is about 1° 5′ north of, and a little following, 8 Cygni, and exactly 1° 30′ south following 4 Cygni.

2^h 16′. The comet is about 40′ from 8 Cygni, in the line between 8 and 4, but rather past the line.

It is about 1° 26′ from 4 Cygni.

VII. *Mr.* Jones's *Computation of the Hyperbolic Logarithm of* 10 *improved: being a Transformation of the Series which he used in that Computation to others which converge by the Powers of* 80. *To which is added a Postscript, containing an Improvement of Mr.* Emerson's *Computation of the same Logarithm. By the Rev.* John Hellins, *Vicar of* Potter's Pury, *in* Northamptonshire. *Communicated by* Nevil Maskelyne, *D.D. F.R.S. and Astronomer Royal.*

Read February 18, 1796.

1. THE method of computing by series is so extensive and useful a part of the mathematics, that any device which facilitates the operation by them will undoubtedly be acceptable to those who are proper judges of these matters. In this persuasion I have employed an hour of that little leisure which my present situation affords me, in improving a calculation of NAPIER's, for finding the hyperbolic logarithm of 10, which was given by the justly celebrated WILLIAM JONES, Esq. F.R.S. in p. 180 of his *Synopsis Palmariorum Matheseos*. The same computation, described in a manner better suited to the capacities of beginners, was also published many years afterward by the learned Dr. SAUNDERSON, in the second volume of his Elements of Algebra, p. 633 and 634. Since Dr. SAUNDERSON's time the doctrine of series has been much improved. My present intention is, to exhibit a transformation of the series by which Mr. JONES computed the hyperbolic logarithm of 10

to others, the terms of which decrease by the powers of 80; so that their convergency is swift, and the divisions by 80 are easily made.

2. Mr. JONES considered the number 10 as composed of $2 \times 2 \times 2 \times \frac{5}{4}$; and consequently obtained the logarithm of 10 by adding three times the logarithm of 2 to the logarithm of $\frac{5}{4}$. The algebraic series which he used on this occasion was $\frac{2d}{s} + \frac{2d^3}{3s^3} + \frac{2d^5}{5s^5} + \frac{2d^7}{7s^7}$, &c. and the numerical value of $\frac{d}{s}$ was $\frac{1}{3}$ for the logarithm of 2, and $\frac{1}{9}$ for the logarithm of $\frac{5}{4}$; so that he has

$$\text{Sum of } \left\{ \begin{array}{l} 3\,\text{L.}\,2 = \frac{6}{3} + \frac{6}{3\cdot 3^3} + \frac{6}{5\cdot 3^5} + \frac{6}{7\cdot 3^7}, \text{ \&c.} \\ \text{L.}\,\frac{5}{4} = \frac{2}{9} + \frac{2}{3\cdot 9^3} + \frac{2}{5\cdot 9^5} + \frac{2}{7\cdot 9^7}, \text{ \&c.} \end{array} \right\} = \text{L. } 10.$$

3. Now the series $\frac{6}{3} + \frac{6}{3\cdot 3^3} + \frac{6}{5\cdot 3^5} + \frac{6}{7\cdot 3^7}$, &c. $(= 3\text{L. } 2)$ is evidently $= \frac{6}{3} \times : 1 + \frac{1}{3\cdot 3^2} + \frac{1}{5\cdot 3^4} + \frac{1}{7\cdot 3^6}$, &c. $= 2 \times : 1 + \frac{1}{3\cdot 9} + \frac{1}{5\cdot 9^2} + \frac{1}{7\cdot 9^3}$, &c. And if the first, third, fifth, &c. term of this series be written in one line, and the second, fourth, sixth, &c. in another, we shall have

$$3\text{L. } 2 = \left\{ \begin{array}{l} 2 \times : 1 + \frac{1}{5\cdot 9^2} + \frac{1}{9\cdot 9^4} + \frac{1}{13\cdot 9^6}, \text{ \&c.} \\ + 2 \times : \frac{1}{3\cdot 9} + \frac{1}{7\cdot 9^3} + \frac{1}{11\cdot 9^5} + \frac{1}{15\cdot 9^7}, \text{ \&c.} \end{array} \right.$$

which two series are evidently

$$= \left\{ \begin{array}{l} 2 \times : 1 + \frac{1}{5\cdot 81} + \frac{1}{9\cdot 81^2} + \frac{1}{13\cdot 81^3}, \text{ \&c.} \\ + \frac{2}{9} \times : \frac{1}{3} + \frac{1}{7\cdot 81} + \frac{1}{11\cdot 81^2} + \frac{1}{15\cdot 81^3}, \text{ \&c.} \end{array} \right.$$

And Mr. JONES's other series, $\frac{2}{9} + \frac{2}{3\cdot 9^3} + \frac{2}{5\cdot 9^5} + \frac{2}{7\cdot 9^7}$, &c. $\left(= \text{L.}\,\frac{5}{4}\right)$ is evidently $= \frac{2}{9} \times : 1 + \frac{1}{3\cdot 9^2} + \frac{1}{5\cdot 9^4} + \frac{1}{7\cdot 9^6}$, &c. $=$

$\frac{2}{9} \times : 1 + \frac{1}{3.81} + \frac{1}{5.81^2} + \frac{1}{7.81^3}$, &c. We therefore now have $3\text{L.}\,2 + \text{L.}\,\frac{5}{4}$ equal to the sum of these three series,

$$2 \times : 1 + \frac{1}{5.81} + \frac{1}{9.81^2} + \frac{1}{13.81^3}, \text{ \&c.}$$

$$\frac{2}{9} \times : \frac{1}{3} + \frac{1}{7.81} + \frac{1}{11.81^2} + \frac{1}{15.81^3}, \text{ \&c.}$$

$$\frac{2}{9} \times : 1 + \frac{1}{3.81} + \frac{1}{5.81^2} + \frac{1}{7.81^3}, \text{ \&c.}$$

which sum is also equal to the hyperbolic logarithm of 10.

4. The form to which Mr. JONES's series are now brought is evidently the same with the general form $a \times : \frac{1}{m} + \frac{x^n}{m+n} + \frac{x^{2n}}{m+2n} + \frac{x^{3n}}{m+3n}$, &c. the value of which, while m and n are affirmative numbers, and x sufficiently small, will be given by the series $a \times : \frac{1}{m.\overline{1-x^n}} - \frac{nx^n}{m.\overline{m+n}.\overline{1-x^n}^2} + \frac{n.2n.x^{2n}}{m.\overline{m+n}.\overline{m+2n}.\overline{1-x^n}^3} - \frac{n.2n.3n.x^{3n}}{m.\overline{m+n}.\overline{m+2n}.\overline{m+3n}.\overline{1-x^n}^4}$,* &c. And this series, if we call the first, second, third, &c. terms of it A, B, C, &c. respectively, and put $\frac{x^n}{1-x^n} = z$, will be more concisely expressed thus; $a \times : \frac{1}{m.\overline{1-x^n}} - \frac{nzA}{m+n} + \frac{2nzB}{m+2n} - \frac{3nC}{m+3n} + \frac{4nzD}{m+4n}$, &c. which form is well adapted to arithmetical calculation.

Now, by comparing the three series at the end of the last article with the general series here given, we shall find that, in the first and last of these series, the value of m is 1, and in the second of them it is 3. The value of n in the first and

* See Phil. Trans. for 1794, Part 2d. p. 218, where this matter is more fully explained.

second series is 4, and in the third it is 2. The values of a are obviously 2 in the first series, and $\frac{2}{9}$ in the second and third. But in each of them z, $= \frac{x^n}{1-x^n}$, is $= \frac{\frac{1}{81}}{1-\frac{1}{81}} = \frac{1}{80}$. These values of the letters being written for them in the second general form, we have three new series, viz.

$$\frac{2.81}{80} - \frac{4A}{5.80} + \frac{8B}{9.80} - \frac{12C}{13.80} + \frac{16D}{17.80}, \text{ \&c.}$$

$$\text{and } \frac{2.81}{9.3.80} - \frac{4A}{7.80} + \frac{8B}{11.80} - \frac{12C}{15.80} + \frac{16D}{19.80}, \text{ \&c.}$$

$$\text{and } \frac{2.81}{9.80} - \frac{2A}{3.80} + \frac{4B}{5.80} - \frac{6C}{7.80} + \frac{8D}{9.80}, \text{ \&c.}$$

which three series are equal in value to those in art. 3, and to the hyperbolic logarithm of 10.

5. With respect to the convergency of these new series, it is evidently somewhat swifter than by the powers of 80. For even in the first series, which has the slowest convergency of the three, the coefficients $\frac{4}{5}$, $\frac{8}{9}$, $\frac{12}{13}$, &c. are each of them less than 1.

6. But another advantage of these new series is, that their numerators and denominators may be reduced to simpler terms, in consequence of which the arithmetical operation by them is further facilitated. In the first and second series, every term after the first is divisible by 4; and every term in the third series admits of a similar reduction by the number 2. The three series then, when these reductions are made, and their first terms are also abbreviated, will stand as below, (each still converging somewhat faster than by the powers of 80); and we shall have the hyperbolic logarithm of

$$= \begin{cases} \frac{81}{40} - \frac{A}{5.20} + \frac{2B}{9.20} - \frac{3C}{13.20} + \frac{4D}{17.20}, \text{ \&c.} \\ \frac{3}{40} - \frac{A}{7.20} + \frac{2B}{11.20} - \frac{3C}{15.20} + \frac{4D}{19.20}, \text{ \&c.} \\ \frac{9}{40} - \frac{A}{3.40} + \frac{2B}{5.40} - \frac{3C}{7.40} + \frac{4D}{9.40}, \text{ \&c.} \end{cases}$$

The arithmetical operation by the new series is undoubtedly easier than by the original series; yet it is evident, by inspection, that half the number of divisions by 20, (although easy operations), in the first and second series, may be exchanged for divisions by 10, which are no more than so many removals of the decimal point; and that, in the third series, half the number of divisions by 40, (the first excepted) may be exchanged for easier ones, one-fourth of them for divisions by 20, and the other fourth for divisions by 10. The new series then, still converging somewhat quicker than by the powers of 80, may stand thus:

$$\frac{81}{40} - \frac{A}{5.20} + \frac{B}{9.10} - \frac{3C}{13.20} + \frac{2D}{17.10}, \text{ \&c.}$$

and $\frac{3}{40} - \frac{A}{7.20} + \frac{B}{11.10} - \frac{3C}{15.20} + \frac{2D}{19.10}$, &c.

and $\frac{9}{40} - \frac{A}{3.40} + \frac{B}{5.20} - \frac{3C}{7.40} + \frac{D}{9.10}$, &c.

And even yet one might still facilitate the computation of the value of some of the terms. Thus, $\frac{3}{20}$ is $= \frac{1 + \frac{1}{2}}{10}$; $\frac{3}{40}$ is $= \frac{1 - \frac{1}{4}}{10}$; $\frac{5}{40}$ is $= \frac{1}{8}$; and $\frac{3}{15.20}$ is $= \frac{1}{100}$, &c.

By these expedients the sum of the three new series, which is equal to the hyperbolic logarithm of 10, may quickly be found.

P. S. *Containing an Improvement of Mr.* EMERSON's *Computation of the Hyperbolic Logarithm of* 10.

7. Since the above paper was written, on looking into EMERSON's Fluxions, I have found, at p. 137 of the first edition,* another computation of the hyperbolic logarithm of 10, which is preferable to Mr. JONES's, on account of the swifter convergency of one of the series used in it, as will appear presently. These series also admit of a transformation to others, by which the constant divisors 81 and 64009, used by Mr. EMERSON, are exchanged for 40 and 32000, while nearly the same rate of convergency is retained; which is another remarkable instance of the utility of transformations of this kind.

Mr. EMERSON, considering the number 10 as composed of $\frac{5^{10}\times 2^{30}}{4^{10}\times 10^9} = \frac{5^{10}}{4^{10}} \times \overline{\frac{1024}{1000}}^3$, and using the same algebraic series as Mr. JONES used on this occasion, finds the hyperbolic logarithm of 10 to be $=$ 10 L. of $\frac{5}{4}$ + 3 L. of $\frac{1024}{1000}$,

$$= \begin{cases} \frac{20}{9} + \frac{20}{3.9.81} + \frac{20}{5.9.81^2} + \frac{20}{7.9.81^3}, \text{ \&c.} \\ + \frac{18}{253} + \frac{18.9}{3.253.64009} + \frac{18.9^2}{5.253.64009^2} + \frac{18.9^3}{7.253.64009^3}, \text{ \&c.} \end{cases}$$

where, instead of a series converging by the powers of $\frac{1}{9}$,† as in Mr. JONES's calculation, we have that which converges by the powers of $\frac{9}{64009}$, or above four times as swiftly. But what renders this very swiftly converging series still more useful is, that it admits of a transformation, by the theorem in article 4,

* See also page 197 of 3d edition.

† See article 3.

to another series which converges by the powers of $\frac{9}{64000}$, by which the numerical calculation is greatly facilitated.

8. For the two series in the preceding article (the sum of which is = H. L. of 10), are evidently =

$$\frac{20}{9} \times : 1 + \frac{1}{3.81} + \frac{1}{5.81^2} + \frac{1}{7.81^3}, \text{ \&c.}$$

and $\frac{18}{253} \times : 1 + \frac{9}{3.64009} + \frac{9^2}{5.64009^2} + \frac{9^3}{7.64009^3}$, &c.

And these two series, when transformed by the theorem abovementioned, and the terms abbreviated, become

$$\frac{9}{4} - \frac{A}{3.40} + \frac{2B}{5.40} - \frac{3C}{7.40} + \frac{4D}{9.40}, \text{ \&c.}$$

and $\frac{9.253}{32000} - \frac{9A}{3.32000} + \frac{2.9B}{5.32000} - \frac{3.9C}{7.32000} + \frac{4.9D}{9.32000}$, &c.

Which series admit of some other abbreviations similar to those pointed out in article 6; and by them may the hyperbolic logarithm of 10 be very easily and expeditiously computed.

June 24, 1795.

VIII. *Manière élémentaire d'obtenir les Suites par lesquelles s'expriment les Quantités exponentielles et les Fonctions trigonométriques des Arcs circulaires.* *Par M.* Simon L'Huilier, *F. R. S.*

Read February 18, 1796.

L'USAGE des logarithmes, et celui des fonctions trigonométriques des arcs de cercle, tels que sont les sinus, cosinus, tangentes, &c. sont si frequens dans les parties les plus élémentaires des mathématiques, soit pures soit mixtes, qu'on doit regarder ces quantités comme apartenant aux élémens; et que leur calcul doit entrer dans un traité élémentaire.

En s'en tenant à la manière ordinaire de présenter les logarithmes dans les élémens, on fait comprendre la possibilité de leur calcul plutôt qu'on ne peut le développer. Outre celui, la dépendance mutuelle des opérations successives par lesquelles on parvient à un resultat approché, exige dans chacune d'elles une exactitude, qui complique et prolonge le calcul, au point qu'il est bien peu de mathématiciens modernes, qui eussent pour les progrès des sciences le dévouement qu'ont eu l'auteur de cette belle découverte, et ceux qui ont poursuivi et complété ce calcul penible, avant qu'on eût trouvé les voïes directes et indépendantes les unes des autres de parvenir aux mêmes résultats.

Les mêmes difficultés et les mêmes longueurs se présentent dans les calculs des fonctions des arcs circulaires, quand on s'en tient aux voïes élémentaires développées jusqu'à présent. Le nombre des extractions de racines, et leur dépendance mutuelle, sont telles, qu'elles exigent un travail affraïant, qui devient superflu, quand le calcul de chaque fonction est rendu direct.

Aussi dans le dévelopement de l'une et de l'autre de ces matières, est-on obligé d'abandonner ces voïes longues et penibles ; et en profitant des tables heureusement déja calculées, ou a coutume de renvoïer aux calculs supérieurs l'exposition des procédés abbregés et directs par lesquels on parvient aux mêmes résultats. Quelques mathématiciens il est vrai, et en particulier EULER dans son *Introductio*, ont exposé ces derniers procédés d'une manière qui paroît les raprocher des élémens. Mais, si on examine avec quelque soin la marche de ce mathématicien, on trouvera qu'elle est entièrement fondée sur le principe de l'infini ; tout au moins trop obscur, pour qu'on puisse regarder comme élémentaires des méthodes auxquelles il sert de base.

J'espère avoir evité ces inconveniens ; et avoir rendu l'exposition de la théorie generale des quantités exponentielles, et des fonctions des arcs circulaires, entièrement élémentaire et indépendante de toute idée de l'infini. La liaison intime qui règne entre les procédés par lesquels je calcule l'une et l'autre de ces espèces de fonctions, est remarquable, et propre à éclaircir l'analogie qui règne entr'elles. Cette analogie, il est vrai, est connue depuis longtems des mathématiciens ; mais, comme elle a été réduite aux expressions imaginaires d'une

de ces espèces de fonctions dans l'autre, elle meritoit d'être présentée d'une manière plus lumineuse.

La méthode que je vais exposer est conforme à celle qu'a emploïée un mathématicien trop modeste et trop peu connu, mon ami M. PFLEIDERER, Professeur à Tubingue, en démontrant le théorème de TAYLOR dans sa dissertation intitulée *Theorematis Tayloriani Demonstratio:* Tubingæ, 1789.

§ 1. *Lemme.* Les différences des puissances des nombres naturels d'un ordre exprimé par l'exposant de ces puissances, est une quantité constante; savoir, le produit continuel des nombres naturels depuis l'unité jusqu'à cet exposant; et partant, les différences des mêmes puissances d'un ordre supérieur évanouissent.

Je pourrois regarder cette proposition comme connue. Mais, comme elle est une des principales de ce mémoire, et que sa démonstration est facile, je crois devoir l'exposer en abrégé.

1. Les différences premières des nombres naturels sont égales à l'unité, et les différences suivantes évanouissent.

2. Les différences premières des nombres quarrés, sont $n^2-(n-1)^2=2n-1$. Donc, les différences secondes des nombres quarrés sont les doubles des différences premières des nombres naturels; et partant 1.2. Et les différences suivantes évanouissent.

3. Les différences premières des cubes, sont $n^3-(n-1)^3=3nn-3n+1$. Donc, les différences troisièmes, qui sont les différences secondes des différences premières, sont les triples des différences secondes des nombres quarrés; savoir 1.2.3; et les différences suivantes évanouissent.

En general. Les différences premières des puissances dont

l'exposant est m, sont $n^m - (n-1)^m = \frac{m}{1} n^{m-1} - \frac{m}{1}, \frac{m-1}{2} n^{m-2} + \frac{m}{1} \ldots \frac{m-2}{3} n^{m-3} \ldots$. Les différences m^{mes} de ces puissances, qui sont les différences $m-1$ des différences premières, sont les différences $m-1^{mes}$ des puissances des nombres naturels dont les exposans sont $m-1$ ou plus petits que $m-1$; affectées de coefficiens constans. Partant, s'il a été prouvé que les différences $m-1^{mes}$ des $m-1^{mes}$ puissances des nombres naturels, sont la quantité constante $1.2.3 \ldots . m-1$; et que les différences du même ordre des puissances inférieures évanouissent; on obtient aussi que les différences m^{mes} des m^{mes} puissances, valent m fois le produit $1.2.3 \ldots m-1$; ou sont le produit $1.2.3 \ldots . m$; et que les différences des ordres supérieurs évanouissent.

Avis. Pour abréger, je désignerai par $\Delta^p \ (\dot{a}^m \ldots - \ldots n^m)$ les différences de l'ordre p des puissances m^{mes} des nombres naturels, depuis a jusqu'à n.

Première Partie. Sur les Logarithmes.

§ 2. *Lemme.* Soit une progression géométrique, commençante par l'unité, *p. ex.* Les différences de tous les ordres des termes de cette progression forment aussi une progression géométrique, dont l'exposant est le même que celui de la première, et dont les termes sont les produits des termes de la première progression par la différence des deux premiers termes elevée à une puissance dont l'exposant est égal à l'ordre de

cette différence. Soit 1, a, a^2, a^3, a^4, a^5 ..... a^{n-1} une progression géométrique :

La suite des différences premières, est

$a-1, a^2-a, a^3-a^2, a^4-a^3, a^5-a^4 \ldots a^{n-1}-a^{n-2}$; ou,

$(a-1)$ $(1, \quad a, \quad a^2, \quad a^3, \quad a^4, \ldots\ldots a^{n-2}.)$ De-là, la suite des différences secondes, est

$(a-1)^2$ $(1, \quad a, \quad a^2, \quad a^3, \quad a^4, \ldots\ldots)$ La suite des differences troisièmes, est

$(a-1)^3$ $(1, \quad a, \quad a^2, \quad a^3, \quad a^4, \ldots\ldots)$ La suite des différences quatrièmes, est

$(a-1)^4$ $(1, \quad a, \quad a^2, \quad a^3 \quad a^4, \ldots\ldots)$

La suite des differences m^{mes}, est

$(a-1)^m$ $(1, \quad a, \quad a^2, \quad a^3, \quad a^4 \ldots\ldots)$

§ 3. *Lemme.* Soit a^z une quantité exponentielle dans laquelle a est plus grande que l'unité. Cette quantité est plus grande que l'unité ou plus petite que l'unité, suivant que z est positif ou négatif; et dans l'un et l'autre cas cette quantité approche de l'unité d'autant plus que z est plus petit: de manière que l'unité est la limite en grandeur ou en petitesse de a^z suivant que z est positif ou négatif.

Corollaire. a^z est une fonction de z de la forme $1 + Az + Bz^2 + Cz^3 + Dz^4 + \ldots$.

§ 4. Soit donc proposée la quantité exponentielle a^z à exprimer dans son exposant z;

Soit $a^z = 1 + Az + Bz^2 + Cz^3 + Dz^4 + Ez^5 + \ldots$.

On aura aussi ...

$$a^{2z} = 1 + 2Az + 2^2Bz^2 + 2^3Cz^3 + 2^4Dz^4 + 2^5Ez^5 + \ldots$$
$$a^{3z} = 1 + 3Az + 3^2Bz^2 + 3^3Cz^3 + 3^4Dz^4 + 3^5Ez^5 + \ldots$$
$$a^{4z} = 1 + 4Az + 4^2Bz^2 + 4^3Cz^3 + 4^4Dz^4 + 4^5Ez^5 + \ldots$$

Soient prises les différences premières ; on aura

$$(a^z - 1)\, a^z = Az + (2^2 - 1)\, Bz^2 + (2^3 - 1)\, Cz^3 + (2^4 - 1)\, Dz^4 + (2^5 - 1)\, Ez^5 + \ldots.$$

$$(a^z - 1)\, a^{2z} = Az + (3^2 - 2^2)\, Bz^2 + (3^3 - 2^3)\, Cz^3 + (3^4 - 2^4)\, Dz^4 + (3^5 - 2^5)\, Ez^5 + \ldots$$

$$(a^z - 1)\, a^{3z} = Az + (4^2 - 3^2)\, Bz^2 + (4^3 - 3^3)\, Cz^3 + (4^4 - 3^4)\, Dz^4 + (4^5 - 3^5)\, Ez^5 + \ldots$$

Or ; puisque $a^z = 1 + Az + Bz^2 + Cz^3 + Dz^4 + \ldots.$

$$a^z - 1 = Az + Bz^2 + Cz^3 + Dz^4 + \ldots.$$

Et les premiers termes de tous les premiers membres des équations précédentes sont Az. Mais, les premiers termes de tous les seconds membres des mêmes équations, sont aussi Az. Donc, on a pour ces premiers termes des expressions identiques, desquelles on ne peut rien déterminer pour la valeur de A. Partant, le coefficient A demeure indéterminé.

Soient prises les différences secondes ; on aura

$$(a^z - 1)^2.\, a^z = \Delta^{ii}(3^2 \ldots 1^2)\, Bz^2 + \Delta^{ii}(3^3 \ldots 1^3)\, Cz^3 + \Delta^{ii}(3^4 \ldots 1^4)\, Dz^4 + \Delta^{ii}(3^5 \ldots 1^5)\, Ez^5 + \ldots$$

$$(a^z - 1)^2\, a^{2z} = \Delta^{ii}(4^2 \ldots 2^2)\, Bz^2 + \Delta^{ii}(4^3 \ldots 2^3)\, Cz^3 + \Delta^{ii}(4^4 \ldots 2^4)\, Dz^4 + \Delta^{ii}(4^5 \ldots 2^5)\, Ez^5 + \ldots$$

$$(a^z - 1)^2.\, a^{3z} = \Delta^{ii}(5^2 \ldots 3^2)\, Bz^2 + \Delta^{ii}(5^3 \ldots 3^3)\, Cz^3 + \Delta^{ii}(5^4 \ldots 3^4)\, Dz^4 + \Delta^{ii}(5^5 \ldots 3^5)\, Ez^5 + \ldots$$

Puisque $a^z - 1 = Az + Bz^2 + Cz^3 + Dz^4 + \ldots\ldots\ldots$; le premier terme de tous les premiers membres des équations précédentes est AAz^2. Mais, le premier terme de tous les seconds membres, est (§ 1) $1.2\, Bz^2$. Donc, égalant les coefficiens des puissances secondes de z, on a $AA = 1.2\, B$; ou, $B = \frac{1}{1.\ldots.2} AA$.

Soient prises les différences troisièmes ; on obtient

$$(a^z-1)^3 a^z = \Delta^{iii}(4^3\ldots1^3)\,Cz^3 + \Delta^{iii}(4^4\ldots1^4)\,Dz^4 + \Delta^{iii}(4^5\ldots1^5)\,Ez^5 + \Delta^{iii}(4^6\ldots1^6)\,Fz^6 + \ldots.$$

$$(a^z-1)^3 a^{2z} = \Delta^{iii}(5^3\ldots2^3)\,Cz^3 + \Delta^{iii}(5^4\ldots2^4)\,Dz^4 + \Delta^{iii}(5^5\ldots2^5)\,Ez^5 + \Delta^{iii}(5^6\ldots2^6)\,Fz^6 + \ldots$$

$$(a^z-1)^3 a^{3z} = \Delta^{iii}(6^3\ldots3^3)\,Cz^3 + \Delta^{iii}(6^4\ldots3^4)\,Dz^4 + \Delta^{iii}(6^5\ldots3^5)\,Ez^5 + \Delta^{iii}(6^6\ldots3^6)\,Fz^6 + \ldots$$

On montre de même ; que, le premier terme des premiers membres de toutes ces équations est $A^3 z^3$; tandis que le premier terme des seconds membres est $1.2.3\,Cz^3$. Donc ; $1.2.3\,C = A^3$; et $C = \frac{1}{1.2.3} A^3$.

Soient prises les différences quatrièmes ; on obtient

$$(a^z-1)^4 a^z = \Delta^{iv}(5^4\ldots1^4)\,Dz^4 + \Delta^{iv}(5^5\ldots1^5)\,Ez^5 + \Delta^{iv}(5^6\ldots1^6)\,Fz^6 \ldots.$$

$$(a^z-1)^4 a^{2z} = \Delta^{iv}(6^4\ldots2^4)\,Dz^4 + \Delta^{iv}(6^5\ldots2^5)\,Ez^5 + \Delta^{iv}(6^6\ldots2^6)\,Fz^6 \ldots.$$

$$(a^z-1)^4 a^{3z} = \Delta^{iv}(7^4\ldots3^4)\,Dz^4 + \Delta^{iv}(7^5\ldots3^5)\,Ez^5 + \Delta^{iv}(7^6\ldots3^6)\,Fz^6 \ldots.$$

Les premiers termes des premiers membres de ces équations sont $A^4 z^4$. Mais, les premiers termes des seconds membres sont $1.2\ldots4\,Dz^4$. Donc ; $1.2..4D = A^4$; et $D = \frac{1}{1.2\ldots4} A^4$.

On montre de la même manière ; en prenant les différences cinquièmes, sixièmes, septièmes . . . . . qu'on a les équations ..... $A^5 = 1.2\ldots5E$; $A^6 = 1.2\ldots6F$; $A^7 = 1.2\ldots7G$ . . . . . et partant ; . . . . . $E = \frac{1}{1.2\ldots5} A^5$; $F = \frac{1}{1.2\ldots6} A^6$; $G = \frac{1}{1.2\ldots7} A^7$. . . . . .

On a donc ; $a^z = 1 + Az + \frac{A^2}{1.2} z^2 + \frac{A^3}{1.2.3} z^3 + \frac{A^4}{1.2...4} z^4 + \frac{A^5}{1.2...5} z^5 + \ldots.$

De même ... $a^{-z} = 1 - Az + \frac{A^2}{1.2} z^2 - \frac{A^3}{1.2.3} z^3 + \frac{A^4}{1.2..4} z^4 - \frac{A^5}{1.2...5} z^5 + \ldots..$

De-là ; .... $\frac{a^z + a^{-z}}{2} = 1 + \frac{A^2}{1.2} z^2 + \frac{A^4}{1.2..4} z^4 + \frac{A^6}{1.2..6} z^6 + \ldots.$

$$\frac{a^z - a^{-z}}{2} = Az + \frac{A^3}{1.2.3} z^3 + \frac{A^5}{1.2..5} z^5 + \ldots..$$

Remarque. On parvient donc par un procédé purement élémentaire, fondé sur une proprieté essentielle et première des progressions géométriques (§ 2.), aux suites qu'on a déduites jusqu'à présent des calculs supérieurs, ou du moins, de l'introduction de l'infini.

Il est connu ; que, *a* est la *base* du système logarithmique ; que A en est le *module*, et faisant $z = 1$, la rélation de *a* à A, est exprimée par l'équation, $a = 1 + A + \frac{A^2}{1.2} + \frac{A^3}{1.2.3} + \frac{A^4}{1.2..4} + \ldots.$ Faisant $A = 1$, le système est celui des logarithmes naturels, dont la base est désignée par *e*. De la dernière suite, on peut exprimer A en *a*, soit par la méthode du retour des suites ; ou plutôt, par la voïe que je développerai bientôt, après avoir fait sur ce qui précède quelques observations.

§ 5. Soit développé le binome $\left(1 + A\frac{z}{n}\right)^n$. On obtient

$$1 + Az + \frac{n}{1}, \frac{n-1}{2} A^2 \frac{z^2}{n^2} + \frac{n}{1}, \frac{n-1}{2}, \frac{n-2}{3}. A^3 \frac{z^3}{n^3} + \frac{n}{1} \ldots \frac{n-3}{4}$$

$$A^4 \frac{z^4}{n^4} + \frac{n}{1} \ldots \frac{n-4}{5} A^5 \frac{z^5}{n^5} + \ldots..$$

$$= 1 + Az + \frac{1-\frac{1}{n}}{2} A^2 z^2 + \frac{1-\frac{1}{n}}{2}, \frac{1-\frac{2}{n}}{3} A^3 z^3 + \frac{1-\frac{1}{n}}{2}, \frac{1-\frac{2}{n}}{3},$$

$$\frac{1-\frac{3}{n}}{4} A^4 z^4 + \frac{1-\frac{1}{n}}{2} \ldots \frac{1-\frac{4}{n}}{5} A^5 z^5 + \ldots\ldots$$

Plus n augmente, plus les facteurs $1 - \frac{1}{n}$, $1 - \frac{2}{n}$, $1 - \frac{3}{n}$, $1 - \frac{4}{n}$ .... approchent d'être égaux à l'unité; et partant, plus n est grand, plus la suite précédente approche d'être

$$1 + Az + \frac{A^2}{1.2} z^2 + \frac{A^3}{1.2.3} z^3 + \frac{A^4}{1.2..4} z^4 + \frac{A^5}{1.2...5} + \ldots;$$ de manière que cette suite est la limite du binome $\left(1 + \frac{Az}{n}\right)^n$.

Donc aussi, la quantité a^z est la limite du binome $\left(1 + \frac{Az}{n}\right)^n$; la quantité a^{-z} est la limite du binome $\left(1 - \frac{Az}{n}\right)^n$; et les quantités $\frac{a^z \pm a^{-z}}{2}$, sont les limites des quantités $\left(1 + A\frac{z}{n}\right)^n \pm \left(1 - A\frac{z}{n}\right)^n$.

§ 6. Je passe à l'expression de z en a et A.

Puisque $a^z = 1 + Az + \frac{A^2}{1.2} z^2 + \frac{A^3}{1.2.3} z^3 + \frac{A^4}{1.2..4} z^4 + \frac{A^5}{1.2...5} z^5 + \ldots.$

Soit $z = n\Delta z$; on aura aussi,

$$a^{\Delta z} = 1 + A\Delta z + \frac{A^2}{1.2} \Delta z^2 + \frac{A^3}{1.2.3} \Delta z^3 + \frac{A^4}{1.2..4} \Delta z^4 + \frac{A^5}{1.2..5} \Delta z^5 + \ldots.$$

$$\text{et } a^z = a^{n\Delta z} = (a^{\Delta z})^n = (1 + A\Delta z + \frac{A^2}{1.2} \Delta z^2 + \frac{A^3}{1.2.3} \Delta z^3 + \frac{A^4}{1.2..4} \Delta z^4 + \frac{A^5}{1.2..5} \Delta z^5 + \ldots)^n. = 1 + v.$$

$$\text{De-là; } (A\Delta z + \frac{A^2}{1.2} \Delta z^2 + \frac{A^3}{1.2.3} \Delta z^3 + \frac{A^4}{1.2..4} \Delta z^4 + \frac{A^5}{1.2..5} \Delta z^5 + \ldots.) = (1 + v)^{\frac{1}{n}} - 1,$$

et $An\Delta z\ (1 + \frac{A}{1.2}\ \Delta z + \frac{A^2}{1.2.3}\ \Delta z^2 + \frac{A^3}{1.2..4}\ \Delta z^3 + \frac{A^4}{1.2..5}$

$\Delta z^4 + \ldots) = n\left((1+v)^{\frac{1}{n}} - 1\right)$

$= (v - \frac{1-\frac{1}{n}}{1.2}v^2 + \frac{1-\frac{1}{n}}{1.2}.\frac{2-\frac{1}{n}}{3}v^3 - \frac{1-\frac{1}{n}}{1.2}.\frac{2-\frac{1}{n}}{3}.\frac{3-\frac{1}{n}}{4}v^4 +$

$\frac{1-\frac{1}{n}}{1.2}\ldots\frac{4-\frac{1}{n}}{5}v^5 - \ldots.)$

Or, (par supp.) n log. $(1 + A\Delta z + \frac{A^2}{1.2}\Delta z^2 + \frac{A^3}{1.2.3}.\Delta z^3 +$

$\frac{A^4}{1.2..4}\Delta z^4 + \frac{A^5}{1.2...5}\Delta z^5 + \ldots) = \log.(1+v)$

ou, n log. $a^{\Delta z} = \log.(1+v)$; et $n\Delta z$ log. $a = \log.\ 1+v$;

ou, faisant a la base du système, et partant log. $a = 1$,

A log. $(1+v)\ (1 + \frac{A}{1.2}\Delta z + \frac{A^2}{1.2.3}\Delta z^2 + \frac{A^3}{1.2.3}\Delta z^3 + \frac{A^4}{1.2..4}$

$\Delta z^4 + \frac{A^5}{1.2..5}\Delta z^5 + \ldots.)$

$= v - \frac{1-\frac{1}{n}}{1.2}v^2 + \frac{1-\frac{1}{n}}{1.2}.\frac{2-\frac{1}{n}}{3}v^3 - \frac{1-\frac{1}{n}}{1.2}.\frac{2-\frac{1}{n}}{3}.\frac{3-\frac{1}{n}}{4}v^4 +$

$\frac{1-\frac{1}{n}}{1.2}.\frac{2-\frac{1}{n}}{3}\ldots.\frac{4-\frac{1}{n}}{5}v^5 - \ldots..$

$= v - \frac{1-\frac{\Delta z}{z}}{1.2}v^2 + \frac{1-\frac{\Delta z}{z}}{1.2}.\frac{2-\frac{\Delta z}{z}}{3}v^3 - \frac{1-\frac{\Delta z}{z}}{1.2}\ldots\frac{3-\frac{\Delta z}{z}}{4}v^4 +$

$\frac{1-\frac{\Delta z}{z}}{1.2}\ldots\frac{4-\frac{\Delta z}{z}}{5}v^5 - \ldots...$

Cette équation aïant toujours lieu; elle a lieu en particulier entre les limites de ses membres; qui

sont A log. $(1+v)$; et $v - \frac{1}{2}v^2 + \frac{1}{3}v^3 - \frac{1}{4}v^4 + \frac{1}{5}v^5 - \ldots..$

donc; A log. $(1+v) = v - \frac{1}{2}v^2 + \frac{1}{3}v^3 - \frac{1}{4}v^4 + \frac{1}{5}v^5 - \ldots..$

A log. $1 - v = -v - \frac{1}{2}v^2 - \frac{1}{3}v^3 - \frac{1}{4}v^4 - \frac{1}{5}v^5 - \ldots..$

A log. $\frac{1+v}{1-v} = 2\,(v + \frac{1}{3}v^3 + \frac{1}{5}v^5 + \ldots..)$

A log. $\sqrt{\frac{1+v}{1-v}} = v + \frac{1}{3}v^3 + \frac{1}{5}v^5 + \ldots.$

Soit $1+v=a$; la base du système

$A=(a-1)-\frac{1}{2}(a-1)^2+\frac{1}{3}(a-1)^3-\frac{1}{4}(a-1)^4+\frac{1}{5}(a-1)^5-\ldots\ldots$

$=\frac{aa-1}{aa+1}+\frac{1}{3}\left(\frac{aa-1}{a+1}\right)^3+\frac{1}{5}\left(\frac{aa-1}{aa+1}\right)^5+\ldots.$ en faisant $\frac{1+v}{1-v}=aa$.

Ce qui est la rélation par laquelle le module est determiné dans la base.

§ 7. Je ne m'arrête pas aux conséquences qui découlent de ces formules connues; mon seul but étant de montrer comment on peut les obtenir par les élémens. Je donnerai pour exemple de leur utilité, la facilité avec laquelle on obtient l'équation différentielle logarithmique; de laquelle réciproquement on a déduit le calcul des logarithmes.

Puisque $a^z=1+Az+\frac{A^2}{1.2}z^2+\frac{A^3}{1.2.3}z^3+\frac{A^4}{1.2..4}z^4+\frac{A^5}{1.2..5}z^5+\ldots.$

$\frac{d.a^z}{dz}=A\left(1+Az+\frac{A^2}{1.2}z^2+\frac{A^3}{1.2..3}z^3+\frac{A^4}{1...4}z^4+\ldots\right)$

$=A.a^z.$

de-là; $\frac{d^2.a^z}{dz^2}=A^2a^z$

$\frac{d^3a^z}{dz^3}=A^3a^z$

$\frac{d^4a^z}{dz^4}=A^4.a^z$

Réciproquement. Puisque $A\log.1+v=v-\frac{1}{2}v^2+\frac{1}{3}v^3-\frac{1}{4}v^4+\frac{1}{5}v^5-\ldots\ldots$

$A\frac{d.\log.1+v}{dv}=1-v+v^2-v^3+v^4-\ldots\ldots=\frac{1}{1+v}.$

SECONDE PARTIE. SUR LES SINUS, COSINUS, ET TANGENTES, DES ARCS DE CERCLE.

§ 8. *Lemmes connus.* 1. La différence des sinus de deux arcs est égale au double produit du cosinus de leur demi-somme par le sinus de leur demi-différence.

2. La différence du cosinus de deux arcs est égale au double produit du sinus de leur demi-somme et de leur demi-différence.

Symboliquement. 1. $\text{Sin. } a - \text{sin. } b = 2 \text{ cos. } \frac{a+b}{2} \text{ sin. } \frac{a-b}{2}$.

2. $\text{Cos. } b - \text{cos. } a = 2 \text{ sin. } \frac{a+b}{2} \text{ sin. } \frac{a-b}{2}$.

§ 9. Soient : Sin. a, sin. $2a$, sin. $3a$, sin. $4a$, sin. $5a$, sin. $6a$, ... les sinus d'arcs en progression arithmétique, croissant, *p. ex.* comme les nombres naturels. Soient prises les différences des ordres successifs de ces sinus ; on obtient

Différences du premier ordre . — $2 \text{ sin. } \frac{1}{2}a$ (cos. $\frac{3}{2}a$, cos. $\frac{5}{2}a$, cos. $\frac{7}{2}a$, cos. $\frac{9}{2}a$, cos. $\frac{11}{2}a$, cos. $\frac{13}{2}a$ . . . .)

Différences du second ordre . . — $2^2 \text{ sin.}^2 \frac{1}{2}a$ (sin. $2a$, sin. $3a$, sin. $4a$, sin. $5a$, sin. $6a$, sin. $7a$ . . . . .)

Différences du troisième ordre . — $2^3 \text{ sin.}^3 \frac{1}{2}a$ (cos. $\frac{5}{2}a$, cos. $\frac{7}{2}a$, cos. $\frac{9}{2}a$, cos. $\frac{11}{2}a$, cos. $\frac{13}{2}a$, cos. $\frac{15}{2}a$ . . . . .)

Différences du quatrième ordre . . + $2^4 \text{ sin}^4 \frac{1}{2}a$ (sin. $3a$, sin. $4a$, sin. $5a$, sin. $6a$, sin. $7a$, sin. $8a$ . . . . .)

Différences du cinquième ordre . . + $2^5 \text{ sin.}^5 \frac{1}{2}a$ (cos. $\frac{7}{2}a$, cos. $\frac{9}{2}a$, cos. $\frac{11}{2}a$, cos. $\frac{13}{2}a$, cos. $\frac{15}{2}a$, cos. $\frac{17}{2}a$ . . . . .)

Différences du sixième ordre . . — $2^6 \text{ sin.}^6 \frac{1}{2}a$ (sin. $4a$, sin. $5a$, sin. $6a$, sin. $7a$, sin. $8a$, sin. $9a$ . . . .)

En général,

Différences du $2m^{me}$ ordre . . . $= 2^{2m} \sin.^{2m} \frac{1}{2} a$ (sin. $m + 1.a$, sin. $m + 2.a$, sin. $m + 3.a$, sin. $m + 4.a$, ...)

Différences du $2m + 1^{me}$ ordre . . $= 2^{2m+1} \sin.^{2m+1} \frac{1}{2} a$ (cos. $\frac{2m+3}{2} a$, cos. $\frac{2m+5}{2} a$, cos. $\frac{2m+7}{2} a$, cos. $\frac{2m+9}{2} a$, ...)

§ 10. De même; soient cos. a, cos. $2a$, cos. $3a$, cos. $4a$, cos. $5a$, cos. $6a$, ... les cosinus d'arcs en progression arithmétique croissant, *p. ex.* comme les nombres naturels. Soient prises les différences des ordres successifs de ces cosinus; on obtient

Différences du premier ordre . . $- 2 \sin. \frac{1}{2} a$ (sin. $\frac{3}{2} a$, sin. $\frac{5}{2} a$, sin. $\frac{7}{2} a$, sin. $\frac{9}{2} a$, sin. $\frac{11}{2} a$, sin. $\frac{13}{2} a$ . . .)

Différences du second ordre . . $- 2^2 \sin.^2 \frac{1}{2} a$ (cos. $2a$, cos. $3a$, cos. $4a$, cos. $5a$, cos. $6a$, cos. $7a$ . . . .)

Différences du troisième ordre . . . $+ 2^3 \sin.^3 \frac{1}{2} a$ (sin. $\frac{5}{2} a$, sin. $\frac{7}{2} a$, sin. $\frac{9}{2} a$, sin. $\frac{11}{2} a$, sin. $\frac{13}{2} a$, sin. $\frac{15}{2} a$ . . .)

Différences du quatrième ordre . . . $+ 2^4 \sin.^4 \frac{1}{2} a$ (cos. $3a$, cos. $4a$, cos. $5a$, cos. $6a$, cos. $7a$, cos. $8a$, ...)

Différences du cinquième ordre . . . $- 2^5 \sin.^5 \frac{1}{2} a$ (sin. $\frac{7}{2} a$, sin. $\frac{9}{2} a$, sin. $\frac{11}{2} a$, sin. $\frac{13}{2} a$, sin. $\frac{15}{2} a$, sin. $\frac{17}{2} a$ . . . . .)

Différences du sixième ordre . . . $- 2^6 \sin.^6 \frac{1}{2} a$ (cos. $4a$, cos. $5a$, cos. $6a$, cos. $7a$, cos. $8a$, cos. $9a$ . . . . .)

En général,

Différences du $2m^{me}$ ordre . . . . . $= 2^{2m} \sin.^{2m} \frac{1}{2} a$ (cos. $m + 1.a$, cos. $m + 2.a$, cos. $m + 3.a$, cos. $m + 4.a$, cos. $m + 5.a$ . . . . .)

Différences du $2m + 1^{me}$ ordre . . . . $= 2^{2m+1} \sin.^{2m+1} \frac{1}{2} a$ (sin. $\frac{2m+3}{2} a$, sin. $\frac{2m+5}{2} a$, sin. $\frac{2m+7}{2} a$, sin. $\frac{2m+9}{2} a$, sin. $\frac{2m+11}{2} a$ . . .)

Remarque. En omettant le facteur $2^m \sin.^m \frac{1}{2} a$; ces suites (de même que la suite fondamentale) reviennent sur elles mêmes, ou sont toujours différentes, suivant que a est commensurable ou non avec la circonférence. Ces suites sont aussi celles des sinus ou cosinus d'arcs qui suivent la progression arithmétique des nombres naturels; seulement, avèc une origine différente.

§ 11. Comme sin. z est zéro, lorsque z est zéro; et qu'outre celui le rapport d'égalité est la limite du rapport d'un arc à son sinus; et que le sinus est plus petit que l'arc; sin. z est une fonction de z de la forme, $z - Az^2 + Bz^3 + Cz^4 + Dz^5 + \ldots\ldots$ Et comme le cosinus d'un arc est l'unité, lorsque z est zéro; cos. z est une fonction de z de la forme $1 - Az + Bz^2 + Cz^3 + Dz^4 + Ez^5 + \ldots\ldots$

§ 12. Soit donc; $\sin. z = z - Az^2 + Bz^3 + Cz^4 + Dz^5 + Ez^6 + \ldots\ldots$

On aura aussi, $\sin. 2z = 2z - 2^2Az^2 + 2^3Bz^3 + 2^4Cz^4 + 2^5Dz^5 + 2^6Ez^6 + \ldots\ldots$

$$\sin. 3z = 3z - 3^2Az^2 + 3^3Bz^3 + 3^4Cz^4 + 3^5Dz^5 + 3^6Ez^6 + \ldots$$

$$\sin. 4z = 4z - 4^2Az^2 + 4^3Bz^3 + 4^4Cz^4 + 4^5Dz^5 + 4^6Ez^6 + \ldots$$

Soient prises les différences premières; on obtient

$$2 \sin. \tfrac{1}{2} z \cos. \tfrac{3}{2} z = z - (2^2 - 1^2) Az^2 + (2^3 - 1^3) Bz^3 + (2^4 - 1^4) Cz^4 + (2^5 - 1^5) Dz^5 + \ldots$$

$$2 \sin. \tfrac{1}{2} z \cos. \tfrac{5}{2} z = z - (3^2 - 2^2) Az^2 + (3^3 - 2^3) Bz^3 + (3^4 - 2^4) Cz^4 + (3^5 - 2^5) Dz^5 + \ldots$$

$$2 \sin. \tfrac{1}{2} z \cos. \tfrac{7}{2} z = z - (4^2 - 3^2) Az^2 + (4^3 - 3^3) Bz^3 + (4^4 - 3^4) Cz^4 + (4^5 - 3^5) Dz^5 + \ldots$$

Réduisant en suites les facteurs du premier membre (§ 11), et exécutant les multiplications ; les premiers termes des produits sont $1z$; et le premier terme de chaque second membre est aussi $1z$; partant, nous apprenons seulement que le premier terme de l'expression du sinus est bien $1z$.

Soient prises les différences secondes ; on obtient

$$-2^2 \sin.^2 \tfrac{1}{2} z \sin. 2z = -\Delta^{ii}(3^2..1^2) Az^2 + \Delta^{ii}(3^3..1^3) Bz^3 + \Delta^{ii}(3^4...1^4) Cz^4 + \Delta^{ii}(3^5...1^5) Dz^5 + ..$$

$$-2^2 \sin.^2 \tfrac{1}{2} z \sin. 3z = -\Delta^{ii}(4^2...2^2) Az^2 + \Delta^{ii}(4^3..2^3) Bz^3 + \Delta^{ii}(4^4...2^4) Cz^4 + \Delta^{ii}(4^5...2^5) Dz^5 + ..$$

$$-2^2 \sin.^2 \tfrac{1}{2} z \sin. 4z = -\Delta^{ii}(5^2...3^2) Az^2 + \Delta^{ii}(5^3..3^3) Bz^3 + \Delta^{ii}(5^4...3^4) Cz^4 + \Delta^{ii}(5^5..,3^5) Dz^5 + ...$$

Or ; le premier terme de chaque premier membre développé en suite conformement aux expressions du § 11me est z^3 ; et les premiers membres ne contiennent pas les secondes puissances de z ; tandis que le coefficient du premier terme Az^2 des seconds membres, qui est la différence seconde des quarrés des nombres naturels, n'évanouit pas. Donc ; dans les seconds membres, le coefficient A de z^2 est zéro.

On démontre de la même manière ; que, dans la suite, $\sin. z = z - Az^2 + Bz^3 + Cz^4 + Dz^5 + Ez^6 + \ldots.$, les coefficiens de toutes les puissances à exposans pairs évanouissent. Savoir ; aïant pris les différences de l'ordre pair zm qui sont $\pm z^{2m} \sin. z^{2m} \tfrac{1}{2} z \sin. pz$ (§ 10.) ; le premier terme du produit des facteurs développés en suites, contient la puissance impaire de z, z^{2m+1} ; de manière que dans ces produits, la puissance paire z^{2m} manque ; donc, elle doit aussi manquer dans les seconds membres : or, le premier terme de chaque second membre contient la puissance paire z^{2m}, avec deux facteurs dont l'un $\Delta^{2m} n^{2m}$ est la quantité constante $1.2 \ldots 2m$ (§ 1.) et

partant n'évanouit pas ; donc, l'autre facteur de cette puissance évanouit. Partant ; le sinus d'un arc est une fonction de cet arc, de la forme sin. $z = z - Az^3 + Bz^5 + Cz^7 + Dz^9 + \ldots$, qui ne contient que les puissances impaires de z.

On a donc ; sin. $z = z - Az^3 + Bz^5 + Cz^7 + Dz^9 + \ldots$

sin. $2z = 2z - 2^3Az^3 + 2^5Bz^5 + 2^7Cz^7 + 2^9Dz^9 + \ldots$

sin. $3z = 3z - 3^3Az^3 + 3^5Bz^5 + 3^7Cz^7 + 3^9Dz^9 + \ldots$

sin. $4z = 4z - 4^3Az^3 + 4^5Bz^5 + 4^7Cz^7 + 4^9Dz^9 + \ldots$

Soient prises les différences troisièmes ; on obtient

$$-2^3 \sin.^3 \tfrac{1}{2} z \cos. \tfrac{5}{2} z = -\Delta^{iii}(4^3 \ldots 1^3)\, Az^3 + \Delta^{iii}(4^5 \ldots 1^5)\, Bz^5 + \Delta^{iii}(4^7 \ldots 1^7)\, C^7 + \Delta^{iii}(4^9 \ldots 1^9)\, Dz^9 + \ldots$$

$$-2^3 \sin.^3 \tfrac{1}{2} z \cos. \tfrac{7}{2} z = -\Delta^{iii}(5^3 \ldots 2^3)\, Az^3 + \Delta^{iii}(5^5 \ldots 2^5)\, Bz^5 + \Delta^{iii}(5^7 \ldots 2^7)\, Cz^7 + \Delta^{iii}(5^9 \ldots 2^9)\, Dz^9 + \ldots$$

$$-2^3 \sin.^3 \tfrac{1}{2} z \cos. \tfrac{9}{2} z = -\Delta^{iii}(6^3 \ldots 3^3)\, Az^3 + \Delta^{iii}(6^5 \ldots 2^5)\, Bz^5 + \Delta^{iii}(6^7 \ldots 3^7)\, Cz^7 + \Delta^{iii}(6^9 \ldots 3^9)\, Dz^9 + \ldots$$

Réduisant en suites les facteurs des premiers membres conformement au § 11 ; les premiers termes de ces membres sont $-z^3$; et les premiers termes des seconds membres sont (§ 1.) $-1.2.3\,A^3$. Donc ; $1 = 1.2.3\,A$; et $A = \frac{1}{1.2.3}$.

Prenant successivement les différences quatrièmes et cinquièmes, on obtient

$$2^5 \sin.^5 \tfrac{1}{2} z \cos. \tfrac{7}{2} z = \Delta^{v}(6^5 \ldots 1^5)\, Bz^5 + \Delta^{v}(6^7 \ldots 1^7)\, Cz^7 + \Delta^{v}(6^9 \ldots 1^9)\, Dz^9 + \ldots$$

$$2^5 \sin.^5 \tfrac{1}{2} z \cos. \tfrac{9}{2} z = \Delta^{v}(7^5 \ldots 2^5)\, Bz^5 + \Delta^{v}(7^7 \ldots 2^7)\, Cz^7 + \Delta^{v}(7^9 \ldots 2^9)\, Dz^9 + \ldots$$

$$2^5 \sin.^5 \tfrac{1}{2} z \cos. \tfrac{11}{2} z = \Delta^{v}(8^5 \ldots 3^5)\, Bz^5 + \Delta^{v}(8^7 \ldots 3^7)\, Cz^7 + \Delta^{v}(8^9 \ldots 3^9)\, Dz^9 + \ldots$$

Réduisant en suites les facteurs des premiers membres (§ 11.) ; les premiers termes de ces membres sont z^5 ; et les

premiers termes des seconds membres sont $1.2\ldots 5Bz^5$. (§ 1.). Donc; $1 = 1.2\ldots 5B$; et $B = \frac{1}{1.2\ldots 5}$.

Prenant de même successivement les différences sixièmes et septièmes; on obtient

$$-2^7 \sin.^7 \tfrac{1}{2}z \cos. \tfrac{2}{2}z = \Delta^{vii}(8^7\ldots 1^7)Cz^7 + \Delta^{vii}(8^9\ldots 1^9)Dz^9 + \Delta^{vii}(8^{11}\ldots 1^{11})Ez^{11} + \ldots$$

$$-2^7 \sin.^7 \tfrac{1}{2}z \cos. \tfrac{11}{2}z = \Delta^{vii}(9^7\ldots 2^7)Cz^7 + \Delta^{vii}(9^9\ldots 2^9)Dz^9 + \Delta^{vii}(9^{11}\ldots 2^{11})Ez^{11} + \ldots$$

$$-2^7 \sin.^7 \tfrac{1}{2}z \cos. \tfrac{13}{2}z = \Delta^{vii}(10^7\ldots 3^7)Cz^7 + \Delta^{vii}(10^9\ldots 3^9)Dz^9 + \Delta^{vii}(10^{11}\ldots 3^{11})Ez^{11} + \ldots$$

Réduisant en suites les facteurs des premiers membres; le premier terme de ces membres est z^7; et les premiers termes des seconds membres sont $1.2\ldots 7Cz^7$. Donc; $C = -\frac{1}{1.2\ldots 7}$.

Prenant de même les différences huitièmes et neuvièmes; on obtient, par les mêmes raisonnemens;

$D = +\frac{1}{1.2\ldots 9}$; puis $E = -\frac{1}{1.2\ldots 11}$; $F = +\frac{1}{1.2\ldots 13}$ .....

Donc, enfin; $\sin. z = z - \frac{1}{1.2.3}z^3 + \frac{1}{1.2..5}z^5 - \frac{1}{1.2\ldots 7}z^7 + \frac{1}{1.2..9}z^9 -$ .....

§ 13. La recherche des cosinus se fait de la même manière.

Soit $\cos. z = 1 - Az + Bz^2 + Cz^3 + Dz^4 + Ez^5 + \ldots$

et partant, $\cos. 2z = 1 - 2Az + 2^2Bz^2 + 2^3Cz^3 + 2^4Dz^4 + 2^5Ez^5 + \ldots$

$\cos. 3z = 1 - 3Az + 3^2Bz^2 + 3^3Cz^3 + 3^4Dz^4 + 3^5Ez^5 + \ldots$

$\cos. 4z = 1 - 4Az + 4^2Bz^2 + 4^3Cz^3 + 4^4Dz^4 + 4^5Ez^5 + \ldots$

Soient prises les différences premières ; on obtient

$$-2\sin.\tfrac{1}{2}z\sin.\tfrac{3}{2}z = -Az + (2^2-1^2)Bz^2 + (2^3-1^3)Cz^3 + (2^4-1^4)Dz^4 + \ldots$$

$$-2\sin.\tfrac{1}{2}z\sin.\tfrac{5}{2}z = -Az + (3^2-2^2)Bz^2 + (3^3-2^3)Cz^3 + (3^4-2^4)Dz^4 + \ldots$$

$$-2\sin.\tfrac{1}{2}z\sin.\tfrac{7}{2}z = -Az + (4^2-3^2)Bz^2 + (4^3-3^3)Cz^3 + (4^4-3^4)Dz^4 + \ldots$$

Développant en suites les facteurs des premiers membres ; les premiers termes de ces membres contiennent les secondes puissances z^2 de z ; et ces membres ne contiennent pas la première puissance de z. Donc ; dans les seconds membres, la seconde puissance de z doit aussi manquer ; donc, $A = 0$. On montre de la même manière, et conformement à ce qui est développé dans le § 12 ; que les puissances de z à exposans impairs manquent dans l'expression du cos. z ; de sorte que, le cosinus est une fonction de z de la forme ;

$$\cos.\ z = 1 - Az^2 + Bz^4 + Cz^6 + Dz^8 + \ldots.$$

et partant,

$$\cos.2z = 1 - 2^2Az^2 + 2^4Bz^4 + 2^6Cz^6 + 2^8Dz^8 + \ldots.$$

$$\cos.3z = 1 - 3^2Az^2 + 3^4Bz^4 + 3^6Cz^6 + 3^8Dz^8 + \ldots.$$

$$\cos.4z = 1 - 4^4Az^2 + 4^4Bz^4 + 4^6Cz^6 + 4^8Dz^8 + \ldots.$$

Soient prises les différences secondes ; on obtient

$$-2^2\sin.^2\tfrac{1}{2}z\cos.2z = -\Delta^{ii}(3^2\ldots.1^2)Az^2 + \Delta^{ii}(3^4\ldots1^4)Bz^4 + \Delta^{ii}(3^6\ldots1^6)Cz^6 + \Delta^{ii}(3^8\ldots,1^8)Dz^8\ldots.$$

$$-2^2\sin.^2\tfrac{1}{2}z\cos.3z = -\Delta^{ii}(4^2\ldots2^2)Az^2 + \Delta^{ii}(4^4\ldots2^4)Bz^4 + \Delta^{ii}(4^6\ldots2^6)Cz^6 + \Delta^{ii}(4^8\ldots2^8)Dz^8\ldots.$$

$$-2^2\sin.^2\tfrac{1}{2}z\cos.4z = -\Delta^{ii}(5^2\ldots3^2)Az^2 + \Delta^{ii}(5^4\ldots3^4)Bz^4 + \Delta^{ii}(5^6\ldots3^6)Cz^6 + \Delta^{ii}(5^8\ldots3^8)Dz^8\ldots..$$

Réduisant en suites les premiers membres de toutes ces équations ; leur premier terme est $-z^2$. Mais, le premier

terme des seconds membres est $-1.2.Az^2$. Donc; $1 = 1.2.A$; et $A = \frac{1}{1.2}$.

Prenant successivement les différences troisièmes et quatrièmes; on obtient

$$2^4 \sin.^4 \tfrac{1}{2} z \cos. 3z = \Delta^{iv}(4^4 \ldots 1^4) Bz^4 + \Delta^{iv}(4^6 \ldots 1^6) Cz^6 + \Delta^{iv}(4^8 \ldots 1^8) Dz^8 + \Delta^{iv}(4^{10} \ldots 1^{10}) Ez^{10} \ldots.$$

$$2^4 \sin.^4 \tfrac{1}{2} z \cos. 4z = \Delta^{iv}(5^4 \ldots 2^4) Bz^4 + \Delta^{iv}(5^6 \ldots 2^6) Cz^6 + \Delta^{iv}(5^8 \ldots 2^8) Dz^8 + \Delta^{iv}(5^{10} \ldots 2^{10}) Ez^{10} \ldots$$

$$2^4 \sin.^4 \tfrac{1}{2} z \cos. 5z = \Delta^{iv}(6^4 \ldots 3^4) Bz^4 + \Delta^{iv}(6^6 \ldots 3^6) Cz^6 + \Delta^{iv}(6^8 \ldots 3^8) Dz^8 + \Delta^{iv}(6^{10} \ldots 3^{10}) Ez^{10} \ldots$$

D'où l'on a de même; $1. = 1.2 \ldots 4B$; et $B = \frac{1}{1.2 \ldots 4}$.

Soient prises successivement les différences cinquièmes et sixièmes; on obtient

$$-2^6 \sin.^6 \tfrac{1}{2} z \cos. 4z = \Delta^{vi}(6^6 \ldots . 1^6) Cz^6 + \Delta^{vi}(6^8 \ldots . 1^8) Dz^8 + \Delta^{vi}(6^{10} \ldots . 1^{10}) Ez^{10} + \ldots$$

$$-2^6 \sin.^6 \tfrac{1}{2} z \cos. 5z = \Delta^{vi}(7^6 \ldots 2^6) Cz^6 + \Delta^{vi}(7^8 \ldots 2^8) Dz^8 + \Delta^{vi}(7^{10} \ldots . 2^{10}) Ez^{10} + \ldots$$

$$-2^6 \sin^6 \tfrac{1}{2} z \cos. 6z = \Delta^{vi}(8^6 \ldots 3^6) Cz^6 + \Delta^{vi}(8^8 \ldots 3^8) Dz^8 + \Delta^{vi}(8^{10} \ldots . 3^{10}) Ez^{10} + \ldots.$$

D'où l'on obtient $-1 = 1.2 \ldots 6C$; et $C = -\frac{1}{1.2 \ldots 6}$.

On obtient successivement . . . . . $D = +\frac{1}{1.2 \ldots 8}$.

$$E = -\frac{1}{1.2 \ldots 10}.$$

Et partant; $\cos. z = 1 - \frac{1}{1.2} z^2 + \frac{1}{1.2..4} z^4 - \frac{1}{1.2 \ldots 6} z^6 + \frac{1}{1.2 \ldots 8} z^8 - \frac{1}{1.2 \ldots 10} z^{10} + \ldots \ldots$

§ 14. Puisque $\sin. z = z - \frac{1}{1.2.3} z^3 + \frac{1}{1.2 \ldots 5} z^5 - \frac{1}{1.2 \ldots 7} z^7 + \frac{1}{1.2..9} z^9 - \ldots \ldots$

et $\cos. z = 1 - \frac{1}{1.2} z^2 + \frac{1}{1.2..4} z^4 - \frac{1}{1.2 \ldots 6} z^6 + \frac{1}{1.2 \ldots 8} z^8 - \ldots$

$$\text{tang. } z \left(= \frac{\text{sin. } z}{\text{cos. } z}\right) = z \times \frac{1 - \frac{1}{1.2.3}z^2 + \frac{1}{1.2..5}z^4 - \frac{1}{1.2..7}z^6 + \frac{1}{1.2...9}z^8 - \ldots\ldots}{1 - \frac{1}{1.2}z^2 + \frac{1}{1.2..4}z^4 - \frac{1}{1.2..6}z^6 + \frac{1}{1.2..8}z^8 - \ldots\ldots}$$

§ 15. Après avoir exprimé le sinus, le cosinus, et la tangente d'un arc, dans cet arc ; on pourroit réciproquement par la méthode du retour des suites, exprimer l'arc dans ces fonctions de lui-même. La méthode suivante est plus élémentaire ; et plus conforme au procédé que j'ai suivi jusqu'à présent.

Il est connu ; que, $\text{cos. } nz = \frac{(\text{cos. } z + \text{sin. } z\sqrt{-1})^n + (\text{cos. } z - \text{sin. } z\sqrt{-1})^n}{2}$

$$\text{sin. } nz = \frac{(\text{cos. } z + \text{sin. } z\sqrt{-1})^n - (\text{cos. } z - \text{sin. } z\sqrt{-1})^n}{2\sqrt{-1}}$$

De-là ; $\text{cos. } nz + \text{sin. } nz\sqrt{-1} = (\text{cos. } z + \text{sin. } z\sqrt{-1})^n$;

$$(\text{cos. } nz + \text{sin. } nz\sqrt{-1})^{\frac{1}{n}} = \text{cos. } z + \text{sin. } z\sqrt{-1}$$

$$\text{cos. } nz - \text{sin. } nz\sqrt{-1} = (\text{cos. } z - \text{sin. } z\sqrt{-1})^n ;$$

$$(\text{cos. } nz - \text{sin. } nz\sqrt{-1})^{\frac{1}{n}} = \text{cos. } z - \text{sin. } z\sqrt{-1}$$

Partant ; $\text{cos. } z = \frac{(\text{cos. } nz + \text{sin. } nz\sqrt{-1})^{\frac{1}{n}} + (\text{cos. } nz - \text{sin. } nz\sqrt{-1})^{\frac{1}{n}}}{2}$.

$$\text{sin. } z = \frac{(\text{cos. } nz + \text{sin. } nz\sqrt{-1})^{\frac{1}{n}} - (\text{cos. } nz - \text{sin. } nz\sqrt{-1})^{\frac{1}{n}}}{2\sqrt{-1}}$$

De-là ; $n \text{ sin. } z = \text{cos. } nz^{\frac{1}{n}} \times (\text{tang. } nz$ Partant aussi ; $n \text{ sin. } \frac{1}{n}z = \text{cos. } z^{\frac{1}{n}} (\text{tang. } z$

$$- \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{2 - \frac{1}{n}}{3} \text{tang.}^3 nz \qquad - \frac{1 - \frac{1}{n}}{1.2} \cdot \frac{2 - \frac{1}{n}}{3} \text{tang.}^3 z$$

$$+ \frac{1 - \frac{1}{n}}{1.2} \ldots \frac{4 - \frac{1}{n}}{5} \text{tang.}^5 nz \qquad + \frac{1 - \frac{1}{n}}{1.2} \ldots \frac{4 - \frac{1}{n}}{5} \text{tang.}^5 z$$

$$- \frac{1 - \frac{1}{n}}{1.2} \ldots \frac{6 - \frac{1}{n}}{7} \text{tang.}^7 nz \qquad - \frac{1 - \frac{1}{n}}{1.2} \ldots \frac{6 - \frac{1}{n}}{7} \text{tang.}^7 z$$

$$+ \frac{1 - \frac{1}{n}}{1.2} \ldots \frac{8 - \frac{1}{n}}{9} \text{tang.}^9 nz \qquad + \frac{1 - \frac{1}{n}}{1.2} \ldots \frac{8 - \frac{1}{n}}{9} \text{tang.}^9 z$$

$$- \ldots\ldots\ldots\ldots \qquad - \ldots\ldots\ldots\ldots$$

$$+ \ldots\ldots\ldots\ldots \qquad + \ldots\ldots\ldots\ldots$$

Donc aussi ; les limites des deux membres de cette équation sont égales entr'elles. Mais, en augmentant n, les limites sont z, et tang. $z - \frac{1}{3}$ tang. $^3z + \frac{1}{5}$ tang. $^5z - \frac{1}{7}$ tang. $^7z + \frac{1}{9}$ tang. $^9z - \ldots\ldots$

Donc ; $z = t - \frac{1}{3}t^3 + \frac{1}{5}t^5 - \frac{1}{7}t^7 + \frac{1}{9}t^9 - \ldots\ldots$ (faisant t = tang. z).

Remarque. Je m'exprime dans ce mémoire d'une manière fort concise sur le passage des quantités variables susceptibles de limites à leurs limites. Je suppose connu que cette théorie peut être dégagée de toute idée de l'infini ; et rappellée à la méthode rigoureuse des anciens connue sous le nom de *Méthode d'Exhaustion.* D'après NEWTON, MACLAURIN, ROBINS, et autres auteurs, j'ai taché de mettre cette théorie à l'abri de toute contestation, dans ma Prix sur l'Infini Mathématique, couronnée par l'Académie de Berlin en 1786. J'espère l'avoir fait de la manière la plus satisfaisante dans l'ouvrage qui s'imprime dans ce moment sous le titre : *Principiorum Calculi differentialis et integralis Expositio elementaris.*

§ 16. Des formules précédentes, on déduit aisément les formules différentielles des fonctions trigonométriques des arcs de cercle.

Puisque sin. $z = z - \frac{1}{1.2.3}z^3 + \frac{1}{1.2..5}z^5 - \frac{1}{1.2...7}z^7 + \frac{1}{1.2...9}z^9 - \ldots\ldots$

$$\frac{d.\sin.z}{dz} = 1 - \frac{1}{1.2}z^2 + \frac{1}{1.2..4}z^4 - \frac{1}{1.2..6}z^6 + \frac{1}{1.2...8}z^8 - \ldots\ldots = \cos.\ z.$$

Puisque cos. $z = 1 - \frac{1}{1.2}z^2 + \frac{1}{1.2..4}z^4 - \frac{1}{1.2..6}z^6 + \frac{1}{1.2..8}z^8 - \ldots\ldots$

$$\frac{d.\cos.z}{dz} = -z + \frac{1}{1.2.3}z^3 - \frac{1}{1.2..5}z^5 + \frac{1}{1.2...7}z^7 - \ldots\ldots = -\sin.z.$$

Puisque tang. $z = \frac{\sin.z}{\cos.z}$; $\frac{d.\text{tang}.z}{dz} = \frac{\cos.z\ \frac{d.\sin.z}{dz} - \sin.z.\ \frac{d.\cos.z}{dz}}{\cos.^2 z}$

$= \frac{\cos.^2 z + \sin.^2 z}{\cos.^2 z} = \frac{1}{\cos.^2 z} = \sec.^2 z.$

C'est ce qu'on pourroit tirer de l'expression

$z = t - \frac{1}{3}t^3 + \frac{1}{5}t^5 - \frac{1}{7}t^7 + \frac{1}{9}t^9$;

$\frac{dz}{dt} = 1 - tt + t^4 - t^6 + t^8 - \ldots = \frac{1}{1+tt} = \frac{1}{\sec.^2 t}$; et partant $\frac{dt}{dz} = \sec.^2 t.$

De-là encore, on déduit les rapports différentiels de tous les ordres successifs.

Savoir ;

$\frac{d.\sin.z}{dz} = \cos.z$	$\frac{d.\cos.z}{dz} = -\sin.z$
$\frac{d^2\sin.z}{dz^2} = -\sin.z$	$\frac{d^2\cos.z}{dz^2} = -\cos.z$
$\frac{d^3\sin.z}{dz^3} = -\cos.z$	$\frac{d^3\cos.z}{dz^3} = +\sin.z$
$\frac{d^4\sin.z}{dz^4} = +\sin.z$	$\frac{d^4\cos.z}{dz^4} = +\cos.z$
$\frac{d^5\sin.z}{dz^5} = +\cos.z$	$\frac{d^5\cos.z}{dz^5} = -\sin.z$

TROISIE'ME PARTIE. SUR L'ANALOGIE ENTRE LES LOGARITHMES ET LES FONCTIONS TRIGONOME'TRIQUES DES ARCS CIRCULAIRES.

§ 17. La ressemblance qui règne entre le procédé par lequel j'ai obtenu les expressions des quantités exponentielles dans leurs exposans, et celui par lequel j'ai obtenu les expressions du sinus et du cosinus d'un arc dans cet arc, me paroît ex-

pliquer de la manière la plus lumineuse l'analogie observée depuis longtems entre les quantités exponentielles et ces fonctions circulaires ; et rendre raison de la conformité des résultats de ces procédés.

Par le § 4. $\frac{e^z + e^{-z}}{2} = 1 + \frac{1}{1.2} z^2 + \frac{1}{1.2..4} z^4 + \frac{1}{1.2...6} z^6 + \frac{1}{1.2...8} z^8 + \frac{1}{1.2...10} z^{10} + \ldots$

Et (§ 13.) $\cos. z = 1 - \frac{1}{1.2} z^2 + \frac{1}{1.2..4} z^4 - \frac{1}{1.2...6} z^6 + \frac{1}{1.2...8} z^8 - \frac{1}{1.2...10} z^{10} + \ldots$

Ces expressions diffèrent seulement par les signes des termes alternatifs, qui contiennent des puissances impairement paires de z; partant, si dans la première on change le signe de zz, en substitant $-zz$ à zz, ou $z\sqrt{-1}$ à z, on obtiendra la seconde ; d'où l'on a été appellé à présenter cos. z sous la forme exponentielle imaginaire, $\cos. z = \frac{e^{+z\sqrt{-1}} + e^{-z\sqrt{-1}}}{2}$.

De même (§ 4.) ; $\frac{e^z - e^{-z}}{2} = z + \frac{1}{1.2..3} z^3 + \frac{1}{1.2..5} z^5 + \frac{1}{1.2...7} z^7 + \frac{1}{1.2...9} z^9 + \ldots\ldots$

Et $\sin. z = z - \frac{1}{1.2.3} z^3 + \frac{1}{1.2...5} z^5 - \frac{1}{1.2...7} z^7 + \frac{1}{1.2...9} z^9 - \ldots\ldots$

Si dans le second membre de la première équation on substitue $z\sqrt{-1}$ à z; et si on divise le résultat par $\sqrt{-1}$; on obtient le second membre de la seconde équation. De-là, on a été appellé à présenter sin. z sous la forme exponentielle imaginaire, $\sin. z = \frac{e^{+z\sqrt{-1}} - e^{-z\sqrt{-1}}}{2\sqrt{-1}}$.

De-là ; tang. $z = \frac{1}{\sqrt{-1}}, \frac{e^{z\sqrt{-1}} - e^{-z\sqrt{-1}}}{e^{z\sqrt{-1}} + e^{-z\sqrt{-1}}}$; et $t\sqrt{-1} = \frac{e^{z\sqrt{-1}} - e^{-z\sqrt{-1}}}{e^{z\sqrt{-1}} + e^{-z\sqrt{-1}}}$.

Donc; $e^{z\sqrt{-1}} : e^{-z\sqrt{-1}} = 1 + t\sqrt{-1} : 1 - t\sqrt{-1}$

$$e^{2z\sqrt{-1}} : 1 \quad = 1 + t\sqrt{-1} : 1 - t\sqrt{-1};$$

$$e^{2z\sqrt{-1}} = \frac{1 + t\sqrt{-1}}{1 - t\sqrt{-1}};\ 2z\sqrt{-1} = \log. \frac{1 + t\sqrt{-1}}{1 - t\sqrt{-1}}.$$

$$z = \frac{1}{2\sqrt{-1}} \log. \frac{1 + t\sqrt{-1}}{1 - t\sqrt{-1}}.$$

Cette formule auroit pu aussi être déduite des deux expressions

$$\tfrac{1}{2} \log. \frac{1 + v}{1 - v} = v + \tfrac{1}{3} v^3 + \tfrac{1}{5} v^5 + \tfrac{1}{7} v^7 + \tfrac{1}{9} v^9 + \ldots.$$

$$z = t - \tfrac{1}{3} t^3 + \tfrac{1}{5} t^5 - \tfrac{1}{7} t^7 + \tfrac{1}{9} t^9 - \ldots\ldots$$

IX. *On the Method of observing the Changes that happen to the fixed Stars; with some Remarks on the Stability of the Light of our Sun. To which is added, a Catalogue of comparative Brightness, for ascertaining the Permanency of the Lustre of Stars.* By William Herschel, *LL. D. F. R. S.*

Read February 25, 1796.

The earliest observers of the stars have taken notice of their different degrees of brilliancy, and, by way of expressing their ideas to others, have classed them into magnitudes. Brightness and size among the stars were taken as synonymous terms, and may still be used as such with sufficient truth, notwithstanding the latter, it seems, can only be looked upon as the consequence of the former. The brightest stars were called of the first magnitude; the next of the second; and those of an inferior lustre of the third, fourth, and fifth magnitudes; and so on.

Among the stars of the first two or three classes there seems to be some natural limit which confines them to a particular order. If we suppose the stars to be about the size of our sun, and at nearly an equal distance from us and from each other, those which form the first inclosure about us will appear brighter than the rest, and there can be only a small number of them. This hypothesis is nearly confirmed by observation, as may be seen by looking over a globe, and applying a pair of compasses opened to 60 degrees, which

should be the angle subtended by the stars of the first magnitude, if they were all scattered equally. For it will be found that the distances from Lyra to Arcturus; from Arcturus to Regulus; from Regulus to Sirius; from Sirius to β Navis; from Elgeuse to Canopus; from Canopus to α Centauri; from α Centauri to Achernar; from Achernar to α Crucis; from Procyon to Canopus; from Fomalhaut to Altair; and from Altair to Antares, agree sufficiently well with this hypothesis. It must also be remembered that a perfect equality in the mutual angular distribution of the stars that form the first inclosure, is a thing that is mathematically impossible, and therefore not to be looked for. This would authorize us to take in other intervals, such as from Arcturus to Antares; from Elgeuse to Regulus; from Achernar to Rigel; from Rigel to Capella; from Capella to Sirius; from Regulus to Spica; from Spica to α Crucis; and from Rigel to Castor; all which concur, in a great measure, to support the same hypothesis. But as the distribution and real magnitude of stars is not my present subject, what has been mentioned will be sufficient.

A second layer of stars will be more extensive; for the superficies of the celestial regions allotted for the situation of these successive stars exceeds the former in the ratio of 4 to 1. And on looking over the collection of stars which astronomers have pointed out as belonging to the second class, we find that their number is proportionally larger.

A similar way of considering the stars of the third order might be applied, if it did not already appear, from what has been said of the two former orders, when strictly compared with the state of the heavens, that such kind of limits can be of no real use in the classification of stars. The hypothesis

of an equality and an equal distribution of stars to which we have referred, is too far from being strictly true to be laid down as an unerring guide in this research. The stars of the first and second class, when scrupulously examined, evidently prove that if we would be accurate, we must admit them, in some degree at least, to be either of different sizes, or placed at different distances. Both varieties undoubtedly take place. This consideration alone is fully sufficient to shew, that how much truth soever there may be in the hypothesis of an equal distribution and equality of stars, when considered in a general view, it can be of no service in a case where great accuracy is required.

Since therefore it appears that in the classification of stars into magnitudes, there either is no natural standard at all, or at least none that can be satisfactory; it follows, that astronomers who have classed them thus, have referred their size or lustre to some imaginary idea of brightness. The great number of stars, indeed, which have been placed into every particular class, may assist us to form a kind of confused type in our minds, by which we may be enabled to arrange others; but how doubtful this must ever remain, we may see from the circumstance of the intermediate expressions that have been introduced.

1.2 m* for instance, denotes that a star so marked is between the first and second magnitude. 2.1 m signifies the same thing, with an intimation that the star so distinguished is nearly of the second magnitude, but partakes still something of the lustre of a star of the first order. With stars of the first, second, and third classes there may be some

* I use the letter m in a short way to express the magnitude of the stars.

necessity to introduce such subdivisions; but how very vague must be the expressions 5m, 5.6m, 6.5m, 6m! In vain have I endeavoured to find a criterion for a star of any one of these magnitudes. On looking over, for instance, the stars of the fifth order, I found that in the list of other stars which ought to be less bright because they were marked 5.6m, 6.5m, or 6m, there were many that exceeded the former in brightness, while among those that are put down 5.4m, 4.5m, or even 4m, which ought to be more bright, I found several of a lustre not equal to some of this fifth magnitude, which I was desirous to ascertain. I may therefore justly call the method that has been hitherto in use to point out the lustre of stars, a reference to an imaginary standard.

The inconvenience arising from this unknown, or at least ill ascertained type to which we are to refer, is such, that now our most careful observations labour under the greatest disadvantage. If any dependence could be placed upon the method of magnitudes, it would follow, that no less than eleven stars in the constellation of the Lion, namely, $\beta\ \sigma\ \pi\ \xi$ A *b c d* 54 48 72, had all undergone a change in their lustre since FLAMSTEED's time. For if the idea of magnitudes had been a clear one, our author, who marked β 1.2m, and γ 2m, ought to be understood to mean that β is larger than γ; but we now find that actually γ is larger than β. Every one of the eleven stars I have pointed out may be reduced to the same contradiction; and as the subject is of some consequence, I shall give a few other instances of them.

σ by FLAMSTEED is 4.5m, $\iota\ \varphi\ \upsilon\ \lambda\ \varkappa\ \pi\ \xi$ are all marked 4m, and therefore ought to be larger; but σ is larger than any of them.

π is marked 4m; *d* 6.5m, χ and *e* 4.5m, *c* and 72 5m; therefore π should be larger than all the former; but it is less.

ξ is marked 4m; but there are eleven stars, namely, σ *b* 54 A *d* χ *e c* 72 27 48 69, all marked in various manners less than that star, yet they all exceed it in magnitude.

Not to proceed any farther with particulars, we ought to account for this by allowing that FLAMSTEED did not compare the stars to each other, but referred each of them separately to its own imaginary standard of magnitude. This is the real source of all such contradictions, which therefore cannot be charged to our author. As we should, however, take it for granted, that the magnitudes were affixed to the stars with as much care as the nature of an unsettled standard would allow, a short inquiry into the extent of the confidence we may place upon the method of magnitudes will be of considerable use.

We have observed that in this method the brightness of stars is referred to unsettled standards; but admitting that a pretty general though coarse idea may be formed of these magnitudes, it may be granted that a mistake of a whole order in the first class cannot be supposed. The difference between a star of the first and second magnitude is so palpable that it excludes all suspicion of taking one for the other.

When subdivisions are introduced, the case becomes doubtful. 1.2m may easily pass for 2.1m. But though these two notations should not be sufficiently clear to be distinguished from each other, yet I am inclined to believe that the former may be precise enough to point out a difference from 2m, and the latter from 2.3m.

With the next order of stars the difference is much less striking; but yet 2m will convey an idea which may be pretty

well distinguished from 3m. 2.3m, however, cannot be sufficiently kept apart from 3.2m, or either of these expressions from 3m, or from 2m. Perhaps the former may be distinguished from 3.4, and the latter from 4m.

The following step from 3m to 4m, or indeed from 3.4m to 4.5m, is less decisive than from 2 to 3m.

Again, if a star had changed from 4m to 5m, or from 4.5m to 5.6m since FLAMSTEED's time, we could hardly entertain more than a very slight suspicion of the alteration. From 4 to 5.6m, or from 4.5 to 6m, would be a pretty considerable step, and might serve as a foundation for an argument.

A change from 5m to 6m is such as no stress could be laid upon; and such are the changes from 5.6 to 6.7m, and from 6 to 7m. In all these inferior orders less than an alteration of a magnitude and an half could hardly deserve attention.

Here we have supposed all references to be made to the same author; for when other astronomers are consulted the uncertainty is much increased. A star which in FLAMSTEED's catalogue stands 1.2m, may be found 2m in another author: 2m in the former may be rated 2.3m, or even 3m by the latter. Of course 3m and 4m may be written for the magnitude of the same star by different persons. 4 and 5m as well as 5 and 6m are frequently interchanged, and no stress can be laid upon such nominal differences in different catalogues. We can hardly allow less than half a magnitude in the higher orders, and a whole one in the inferior classes, for this uncertainty.

To apply what has been said: suppose there should be some inducement to believe a certain star, such as β Leonis, to have changed its lustre. Now having no real, existing type of

comparison, we can only refer to the general, imaginary one; and here the rules we have laid down will be of considerable service. The magnitude of this star given by FLAMSTEED is 1.2m; but as we have shewn that there is some ground to admit that 1.2m, even in this coarse way of reference, may be distinguished from what the same author seems to have taken for 2m, we conclude that the star has probably lost some of its former brightness. Again, he gives β 1.2m, and γ 2m. This notation may be taken to imply, though indirectly, that β is larger than γ; which not being the case, we have an additional reason to suspect a change. DE LA CAILLE puts down β 2m. Now the difference between the notation 1.2m of FLAMSTEED and 2m of the latter author, can add nothing to the force of the argument for a change; as we have observed before, that a considerable allowance must be made for nominal varieties in different authors. Nor can we draw any support from the magnitude itself, because the star will pass very well for one of that order, when compared with other stars which are marked 2m by the same author. But when DE LA CAILLE marks β 2m, and γ 3m, we may then conclude that he estimated β to be larger than γ, though we do not know that he compared these two stars together; because a whole magnitude in the second class, as we have said, cannot well be mistaken, coarse as is the type to which the reference is made. Upon the whole, therefore, we conclude that β Leonis is now less brilliant than it was formerly.

In this manner, with proper circumspection, we may get at some certainty, even by the method of magnitudes; the imperfection of it, however, in other cases is very obvious. σ Leonis, for instance, being marked by FLAMSTEED 4.5m, the

star itself will in every respect pass for one of that magnitude, when compared to a mental standard taken from other stars of the same author. Nor can its being brighter than stars which have a magnitude of a superior lustre affixed to them, do more than raise a considerable suspicion of a change. But as this subject will occur again hereafter, and as it must be sufficiently apparent that the present method of expressing the brightness of the stars is very defective, we now proceed to propose a different one.

I place each star, instead of giving its magnitude, into a short series, constructed upon the order of brightness of the nearest proper stars. For instance, to express the lustre of D, I say CDE. By this short notation, instead of referring the star D to an imaginary uncertain standard, I refer it to a precise, and determined, existing one. C is a star that has a greater lustre than D; and E is another of less brightness than D. Both C and E are neighbouring stars, chosen in such a manner that I may see them at the same time with D, and therefore may be able to compare them properly. The lustre of C is in the same manner ascertained by BCD; that of B by ABC; and also the brightness of E by DEF; and that of F by EFG.

That this is the most natural, as well as the most effectual way to express the brightness of a star, and by that means to detect any change that may happen in its lustre, will appear, when we consider what is requisite to ascertain such a change. We can certainly not wish for a more decisive evidence, than to be assured, by actual inspection, that a certain star is now no longer more or less bright than such other stars to which it has been formerly compared; provided we are at the same

time assured that those other stars remain still in their former unaltered lustre. But if the star D will no longer stand in its former order CDE, it must have undergone a change; and if that order is now to be expressed by CED, the star has lost some part of its lustre; if on the contrary, it ought now to be denoted by DCE, its brightness must have had some addition. Then, if we should doubt the stability of C and E, we have recourse to the orders BCD, and DEF, which express their lustre; or even to ABC, and EFG, which continue the series both ways. Now having before us the series BCDEF, or if necessary even the more extended one ABCDEFG, it will be impossible to mistake a change of brightness in D, when every member of the series is found in its proper order, except D.

Here I have used the letters of the alphabet merely to explain my way of fixing the order of brightness of the stars. In the journal or catalogue itself, which gives this order of brightness, each star must bear its own proper name, or number. For instance, the brightness of the star δ Leonis may be expressed by $\beta\ \delta\ \varepsilon$ Leonis, or better by 94 — 68 — 17 Leonis; these being the numbers which the three above stars bear in the British catalogue of fixed stars.

Perhaps it may be thought that the known introduction of letters, added to the magnitudes of the stars, seems to be that very method which I now recommend, as different from what has already been used. And certainly if letters had been annexed to stars with a strict view to their order of brightness, they would now be of considerable service; but the intention of the astronomers who lettered the stars seems only to have been to give them a name, whereby to call them more readily, than by the descriptive method of pointing out their situation.

It was indeed natural enough to give the name α to the brightest star, on account of its being the most remarkable in a constellation; and we may admit that with a few of the most conspicuous stars the letters $\alpha\beta\gamma$ would present themselves in succession; but whoever compares all the letters of the Greek and English alphabet that have been used, with the numerical magnitudes annexed to the same stars, will immediately give up all thoughts of intended order. In the constellation of Andromeda, which happens to lie before me, I find the following arrangement: $\delta o \mu \varepsilon$, $\theta \pi \xi$, $\lambda \upsilon \upsilon \lambda$, and *d b c*. In that of Hercules $\varepsilon \delta$, $\xi \lambda \varkappa$, $\pi \theta$, $\varrho \mu$, $\sigma \nu$, τo, and *h* A *e b k q c m* Z.

It will be needless to point out the irregularities which take place in every other constellation; they go indeed so far, that it would be wrong to call them irregularities, because certainly no order could be intended in the arrangement of the letters. A doubt has even arisen whether any succession of brightness might be argued from the very first, second, or third letters of the alphabet; and when we find them arranged thus: $\beta\ \alpha$ Cassiopeæ, $\beta\ \alpha$ Cancri, $\gamma\ \beta$ Aquilæ, $\beta\ \zeta$ Canis minoris, 41 γ Arietis, we can hardly think it safe to regard the order of letters as of the least consequence. To which may be added, that in many constellations $\alpha\beta\gamma$ are all marked to be of the same magnitude, in which case again the order of the letters can bring no information. And therefore, even in those cases where the order of the letters agrees with the different magnitudes assigned to them, the knowledge we can have of the former state of the heavens must be derived from the magnitudes, and cannot be from the letters.

It may in the next place be remarked, that if not the

letters, at least the numerical magnitudes affixed to the stars by astronomers, point out an order of brightness; and therefore contain my method already established. A succession of the marks 1, 2, 3, 4, 5, *&c.* and other intermediate notations, which are to be found in the British, and other catalogues, give us a long list of stars that are (or should be) in a regular order of brightness, from a star of the first magnitude down to one of the eighth or ninth.

That these marks, denoting the magnitudes of the stars, are of some use every astronomer will readily perceive; but if we would apply them to the purpose of detecting a change in the lustre of some suspected star, the defect of this method will easily appear, and has already been shewn in the instance of σ Leonis. It was hinted before that the subject would recur again, I shall therefore mention two other instances, in the first of which the common notation is sufficiently expressive. It will be so in all cases where a very considerable change takes place. Thus, β Persei being marked 2.3m, and ϱ of the same constellation 4m, there could be no doubt of a change in the light of Algol when it was found to be not brighter than ϱ. But let us in the next place take an observation recorded in my journal.

"May 12, 1782. β Lyræ is much less than γ."

Now, examining the British catalogue, we find β 3m, and γ 3m. Had the method of orders been adopted by FLAMSTEED, we should at once have pronounced this star to be changeable. For it would have been $\beta\,\gamma$ in his time, and $\gamma\,\beta$ at the time of observation; but since we have shewn that no inference can be drawn from the order of the letters, we have only the magnitudes to refer to. And here again the deviation

of β from its usual brightness not being so considerable, but that a star such as it appeared to be at the time of observation might pass for one of the third magnitude, we are left in the dark; notwithstanding which a few years after, this star was actually found to be not only changeable, but periodical.*

M. DE LA LANDE in mentioning the change of δ Ursæ majoris arranges the seven bright stars of that constellation as they appeared to him; and remarks that sometimes γ and ε should stand before β, and sometimes after it. Here we have something like an order of seven remarkable stars; but as it happens, the stars themselves are not favourable to the formation of a regular series. Mr. PIGOTT and Mr. GOODERICKE also compared the stars whose changes they were examining to other neighbouring stars that were proper to be estimated with them, and were in a manner forced to lay aside the method of magnitudes.† These instances contribute to support the arguments I have used, to shew that another method of ascertaining the lustre of the stars is required, while at the same time they sufficiently indicate that the comparative brightness of stars is the only safe one to which we can have recourse.

It will be necessary now to enter into a full display of my proposed method; for simple as it is in its principle, it is not only difficult but very laborious in its progress. I began to put it into execution about 14 years ago; but other very interesting astronomical pursuits have broken in upon the regular continuation of it. By relating the difficulties or inconveniences as they happened, it will appear that my present

* Phil. Trans. Vol. LXXVI. Part I. page 197.

† Phil. Trans. Vol. LXXV. Part I. page 127 and 154.

letters, at least the numerical magnitudes affixed to the stars by astronomers, point out an order of brightness; and therefore contain my method already established. A succession of the marks 1, 2, 3, 4, 5, *&c.* and other intermediate notations, which are to be found in the British, and other catalogues, give us a long list of stars that are (or should be) in a regular order of brightness, from a star of the first magnitude down to one of the eighth or ninth.

That these marks, denoting the magnitudes of the stars, are of some use every astronomer will readily perceive; but if we would apply them to the purpose of detecting a change in the lustre of some suspected star, the defect of this method will easily appear, and has already been shewn in the instance of σ Leonis. It was hinted before that the subject would recur again, I shall therefore mention two other instances, in the first of which the common notation is sufficiently expressive. It will be so in all cases where a very considerable change takes place. Thus, β Persei being marked 2.3m, and ϱ of the same constellation 4m, there could be no doubt of a change in the light of Algol when it was found to be not brighter than ϱ. But let us in the next place take an observation recorded in my journal.

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notation, as well as method of arranging the observations, are liable to the fewest objections.

The general disposition of the stars is in constellations. This order is to be preferred to that of right ascension, or polar distance, because the stars being to be compared to the nearest proper stars that can be found, the constellations themselves will generally answer that purpose better than other selections.

My first design was to draw each whole constellation into one series. Accordingly I began July 16, 1781, to arrange the stars in Ophiuchus thus:

"Order of the stars in Ophiuchus; $\alpha\,\beta\,\delta\,\zeta\,\eta\,\varkappa\,\gamma\,\varepsilon$."

This way of placing the stars agrees so far with my present one, that any star, such as $\varkappa$ for instance, may be taken, and the expression of its lustre will be had by $\eta\,\varkappa\,\gamma$. And as FLAMSTEED marks the magnitudes of these stars 3m 4m 3m, my arrangement does not agree with his. If we should now suspect $\varkappa$ to have changed its lustre, recourse may be had to another star on both sides, which gives $\zeta\,\eta\,\varkappa\,\gamma\,\varepsilon$. The magnitudes of FLAMSTEED are 3m 3m 4m 3m 3.4m, where $\varkappa$ again seems to be placed in a situation to which it is not intitled.

A defect of this arrangement, which was not immediately perceived, is that in taking the stars of a constellation we have not always a proper connection of the steps of the series that may be formed of them: there being too much difference in the lustre of some of the stars, and too little in others.

Other inconveniences will also arise from the multiplicity of the members of a general series, and the trouble of arranging them when they are nearly equal. To get over these difficulties I marked the stars that differed much in lustre by

magnitudes or degrees of difference; in which I assumed three different sorts of each; namely, $1'\ 1''\ 1'''\ 2'\ 2''\ 2'''$, &c. For instance,

"May 12, 1783. Order of the stars in Bootes;

"$\alpha 1'\ \varepsilon 2''\ \eta 2'''\ \gamma\beta\delta 3'\ \varrho 3''\ \zeta 3'''\ \pi 4$."

That this is not recurring to the old method of magnitudes, will appear when we consider that the stars are strictly compared. The series $\alpha\varepsilon\eta\gamma\beta\delta\varrho\zeta\pi$ remains established, but the difference in the gradation of brightness between the members of the series is added to it. At first this seemed to answer the intended purpose; for $\alpha\varepsilon\eta$ not being sufficiently distinguished, the addition $1'$ to α, and $2''$ to ε, shewed that α was very much brighter than ε, while $2'''$ added to η denoted only a very small difference between this and ε. The difficulty which immediately after arose in the choice of the magnitudes, however, soon convinced me that the fallacy of them would still have some influence upon the arrangements. The same evening I marked the stars in Leo thus:

"Order of the stars in Leo;

"$\alpha 1'''\ \gamma 2'\ \beta 2'\ \delta\varepsilon\ \zeta\theta\eta\ \mu o\ \varrho\nu\sigma$."

Here I parcelled them together in the order of brightness, but could not find a convenient way to denote the different degrees by using any derivation from magnitudes; therefore I contented myself with placing those close together that agreed nearly with each other, and kept a little distance between those that differed rather more. This might perhaps have answered the required end, if the confusion which would arise from the distance of letters had not proved a great objection. And that it would unavoidably bring on mistakes we

may see by the other constellations which were arranged that evening.

"Draco γ η β δ ζ, θ λ α κ ξ

"Cygnus α γ ε β δ ζ θ

"Hercules β ζ α δ η π γ ε μ r* changed."

August 16, 1783, being upon the same subject of assigning comparative magnitudes, I introduced lines to shew the intended distances of the letters, with a view to prevent mistakes that might be made in transcribing them, and expressed the order as follows:

"Order of the stars in Auriga;

"α —— γ β —— ι θ —— ε η ζ —— υ π τ"

The marks denoted that all the stars were in succession, but that the distance between those which are separated by lines was greater than that between the rest. When stars occurred that were nearly equal, I placed them under each other, thus:

"Order of the stars in Ursa Minor, α β —— γ —— δ ε ζ

But in this expression there is the inconvenience of its breaking in upon the lines above and below.

Another cause of disorder arose from the stars which are not lettered. For here we are obliged to use numbers in lieu of them; and these, unless properly separated, will run into one another, and occasion mistakes.

* I called it *r* changed, because this star, which in my edition of 1725 is marked 3 m, is only of the 5th magnitude. At that time I ascribed the difference to a change in the star; but I have since found that there is an error in the edition of 1725 which is not in that of 1712, where the star is marked as it ought to be, of the 5th magnitude.

In the next place, the letters themselves became troublesome; for a star cannot be found so readily in a catalogue or in an atlas by a letter, as it may be by a number.

The inconveniences attending the above different ways of notation having now been sufficiently pointed out, it remains only to lay down the method upon which, after many trials, I have fixed, in order to avoid them.

Setting aside the letters entirely I use only numbers in all my observations, and these numbers are such as I have added with red ink both to the edition of 1725 of the British catalogue, and to the Atlas Cœlestis taken from that catalogue, and printed in 1729. When I use other stars than what are contained in the British catalogue, the authors who have given them, and their numbers in the catalogues from whence they are taken, are particularly mentioned.

In the choice of the stars which are to express the lustre of any particular one, my first view is directed to a perfect equality. When two stars are perfectly alike in brightness, so that by looking often and a long while at them, I either cannot tell which is the brightest, or occasionally think one the largest, and sometimes, not long after, give the preference to the other, I put down their numbers together, only separated by a point. For instance, 30 . 24 Leonis. However, it can happen but very seldom that the equality in the lustre of two neighbouring stars is so perfect as not to leave an inclination to prefer one to the other; therefore I place that first which may probably be the largest, even though I do not particularly judge it to be so. But this preference is never to be understood to extend so far as to make it improper to change the order of the two stars; and the expression 24 . 30 Leonis will be equally good with the

former. When a third star is concerned, such as 30.24.77 Leonis, the order of them ought not to be changed; notwithstanding an equality between each member of the series has been strictly ascertained. The reason of this is obvious. For by the order in which they are placed, it appears that 30 has been deemed equal to 24, and 24 equal to 77; but it is not affirmed that 30 has been compared to 77. There will be a great probability that these two last stars do not differ sensibly or materially; but since actual comparison is what we are to go by, the order in which the stars are given must remain.

When two stars are so nearly alike in their lustre that they may be almost called equal, and even now and then leave us doubtful to which to give the preference; but when upon a longer inspection of them we always return to decide it in favour of the same, I separate the numbers that denote these stars by a comma. For instance, 41, 94 Leonis. This expression can certainly not be changed to 94, 41 Leonis; much less can the order of three such stars, as 20, 40, 39 Libræ, admit of a different arrangement. If ever the state of the heavens should be such as to require a different order in these numbers, we need not hesitate a moment to declare a change in the brightness of one or more of the stars that are contained in the series to have taken place.

When two stars differ but very little in brightness, but so that even a doubt cannot arise to which the preference ought to be given, I separate the numbers by which they are to be found in the catalogue by a short line. For instance, 17 – 70 Leonis; or 68 – 17 – 70 Leonis. If, in the former instance, a breaking in upon the order is to be looked upon as a proof that at least one of the stars has undergone a change in its

lustre, much more must that change be evident in this case, where the stars are separated by lines instead of commas.

When two stars differ so much in brightness that one or two other stars might be put between them, and still leave sufficient room for distinction, they become partly unfit for standards by which the lustre of other stars can be ascertained. But as proper intermediate stars sometimes cannot conveniently be had, we are often obliged to retain them; and in that case I distinguish them by a line and comma –, or by two lines, as 32 – – 41 Leonis. A difference which exceeds those that are expressed by the above marks, I denote by a broken line, thus – – –, for instance, 16 – – – 29 Bootis. It would be very easy to give a more extensive signification to lines by adding cross marks to them, such as, + ⫞⫟ ⫞⫟⫟ ⫞⫟⫟⫟ *&c.;* but in estimations that are to ascertain the brightness of stars, such expressions would rather throw us back again to look for imaginary differences, resembling those which have been rejected in the old system of magnitudes. On the contrary, the marks I have introduced admit of so precise a definition, that they cannot possibly be mistaken: a point denoting equality of lustre: a comma indicating the least perceptible difference: a short line to mark a decided but small superiority: a line and comma, or double line, to express a considerable and striking excess of brightness; and a broken line to mark any other superiority which is to be looked upon as of no use in estimations that are intended for the purpose of detecting changes.

In a foregoing paragraph we have said that this method of ascertaining the lustre of the stars was difficult and laborious. The difficulty consists in avoiding the various causes of error

that may bias our judgment in assigning the comparative brightness of the stars: the different altitudes at which we view them: the state and situation of the moon: the time of the night with regard to twilight: the uncertainty of flying clouds: the twinkling and continual change of star-light, to whatever cause it may be owing; I mean such changes as last but few moments, or at most but a few minutes: a return into the dark after having been writing by candle-light: the zodiacal light: aurora borealis: and dew or damp upon the glasses or specula when a telescope is used. All these, it must be confessed, are real difficulties, which it requires much attention and perseverance to get the better of.

That the method is also laborious may be easily conceived; for each star must at least have two other stars to be compared with, and even these will often be found not to be sufficient. To look out for such proper objects, and then to make the necessary comparisons for every star in the heavens, can be no easy task, especially when we remember the difficulties I have enumerated, to which every single estimation of comparative brightness is subject. This ought, however, not to discourage us from a work which has in view the investigation of a point of great importance; and as I have already made a considerable progress, I shall give the result of my labour in small catalogues, of which I have joined one at the end of this paper.

That these investigations are of the importance we have ascribed to them, will appear when we call to our remembrance the great number of alterations of stars that we are certain have happened within the last two centuries, and the much greater number that we have reason to suspect to have

taken place. If we consider how little attention has formerly been paid to this subject, and that most of the observations we have are of a very late date, it would perhaps not appear extraordinary were we to admit the number of alterations that have probably happened to different stars to be a hundred; this compared with the number of stars that have been examined, with a view to ascertain their changes, which we can hardly rate at three thousand, will give us a proportion of 1 to 30. But we are very certain that had a number of observers applied themselves to the same subject, which is of such a nature as to require the attentive scrutiny of many diligent persons at the same time, many more discoveries might probably have been made of changeable and periodical stars, whose variations are too small to strike a general observer. In the application we shall make of this subject however, a proportion, such as 1 to 30, or even 1 to 300, is sufficiently striking to draw our attention.

By observations such as this paper has been calculated to promote and facilitate, we are enabled to resolve a problem not only of great consequence, but in which we are all immediately concerned. Who, for instance, would not wish to know what degree of permanency we ought to ascribe to the lustre of our sun? Not only the stability of our climates, but the very existence of the whole animal and vegetable creation itself is involved in the question. Where can we hope to receive information upon this subject but from astronomical observations? If it be allowed to admit the similarity of stars with our sun as a point established, how necessary will it be to take notice of the fate of our neighbouring *suns*, in order to guess at that of our own! That *star* which among the multi-

that may bias our judgment in assigning the comparative brightness of the stars: the different altitudes at which we view them: the state and situation of the moon: the time of the night with regard to twilight: the uncertainty of flying clouds: the twinkling and continual change of star-light, to whatever cause it may be owing; I mean such changes as last but few moments, or at most but a few minutes: a return into the dark after having been writing by candle-light: the zodiacal light: aurora borealis: and dew or damp upon the glasses or specula when a telescope is used. All these, it must be confessed, are real difficulties, which it requires much attention and perseverance to get the better of.

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tude we have dignified by the name of *sun*, to-morrow may slowly begin to undergo a gradual decay of brightness, like β Leonis, α Ceti, α Draconis, δ Ursæ majoris, and many other diminishing stars that will be mentioned in my catalogues. It may suddenly increase, like the wonderful star in the back of Cassiopea's chair, and the no less remarkable one in the foot of Serpentarius; or gradually come on like β Geminorum, β Ceti, ζ Sagittarii, and many other increasing stars, for which I also refer to my catalogues. And lastly, it may turn into a periodical one of 25 days duration, as Algol is one of 3 days, δ Cephei of 5, β Lyræ of 6, η Antinoi of 7 days, and as many others are of various periods.

Now, if by a proper attention to this subject, and by frequently comparing the real state of the heavens with such catalogues of brightness as mine, it should be found that all, or many of the stars which we now have reason to suspect to be changeable, are indeed subject to an alteration in their lustre, it will much lessen the confidence we have hitherto placed upon the permanency of the equal emission of light of our sun. Many phænomena in natural history seem to point out some past changes in our climates. Perhaps the easiest way of accounting for them may be to surmise that our sun has been formerly sometimes more and sometimes less bright than it is at present. At all events, it will be highly presumptuous to lay any great stress upon the stability of the present order of things; and many hitherto unaccountable varieties that happen in our seasons, such as a general severity or mildness of uncommon winters or burning summers, may possibly meet with an easy solution in the real inequality of the sun's rays.

A method of ascertaining the quantity or intenseness of solar light might be contrived by some photometer or instrument properly constructed, which ought probably to be placed upon some high and insulated mountain, where the influence of various causes that affect heat and cold, though not entirely removed, would be considerably lessened. Perhaps the thermometer alone might be sufficient. For though the lustre of the sun should be the chief object of this research, yet, as the effect of light in producing expansion in mercury seems to be intimately connected with the quantity of the incident solar rays, it may be admitted that all conclusions drawn from their action upon the thermometer will apply to the investigation of the brilliancy of the sun. And here the forms laid down by Mr. MAYER, in his little treatise *De Variationibus Thermometri accuratius definiendis*,* may be of considerable service to distinguish the regular causes of the change of the thermometer from the adventitious ones, among which I place the probable instability of the sun's lustre.

Introductory Remarks and Explanations of the Arrangement and Characters used in the following Catalogue.

This catalogue contains nine constellations, which are arranged in alphabetical order. I have called the present collection the first catalogue. The rest of the constellations, which are pretty far advanced, will be given in successive small catalogues as soon as time will permit to complete them.

Each page is divided into four columns, the first of which gives the number of the stars in the British catalogue of Mr. FLAMSTEED, as they stand arranged in the edition of 1725.

* *Tobiæ Mayeri opera inedita*, I.

The second column contains the letters which have been affixed to the stars.

The third column gives the magnitude assigned to the stars by FLAMSTEED in the British catalogue; and

The fourth contains my determination of the comparative brightness of each star, by a reference to proper standards.

All numbers used in the fourth column refer to the stars of the same constellation in which they occur, except when they are marked by the name of some other constellation; and in that case the alteration so introduced extends only to the single number which is marked, and which then refers to the constellation affixed to the number.

The numbers at the head of the notes, which will be found at the end of the catalogue, refer to the stars in the same constellation to which the notes belong. They contain particulars which it will be useful to know for those who wish to review that constellation.

To each star which I could not find in the heavens, and which, upon examining FLAMSTEED's observations, appeared never to have been seen by him, I have put down "Does not exist." To such as I could not find in the heavens, but which nevertheless appeared to have been observed by FLAMSTEED, I have put down "Lost." This is to be understood only to mean that the star in question was not to be seen when I looked for it, but that possibly at some future time, if it be a changeable or periodical star, it may come to be visible again.

The observations in the notes, distinguished by marks of quotation, " " are taken from my own journals.

Errors in FLAMSTEED's catalogue, or in the Atlas Cœlestis,

are pointed out at the end of the constellations in which they occur, that they may be corrected.

Simple Characters.

‘ The least perceptible difference less bright.
. Equality.
, The least perceptible difference more bright.
– A very small difference more bright.
–, A small difference more bright.
– – A considerable difference more bright.
– – – Any great difference more bright in general.

Compound Characters, expressing the wavering of Star-light.

: From the least perceptible difference less bright to equality.

; From equality to the least perceptible difference more bright.

⁻; From a very small difference more bright, to the least perceptible difference.

=’ From –, to – *&c.*

⁘ The wavering expressed by the passing of the light from a state of the least perceptible difference less bright to equality, and to the least perceptible difference more bright.

⁖ The wavering expressed by the changes from – to , and to . or from . to , and to –

General Characters.

= Perfect equality.
< Less, but undetermined.
> Larger, but undetermined.

All the observations contained in this catalogue have been made in very fine nights, where no suspicion of any whitish haziness or thin clouds can be admitted that might have deceived me.

The compound expressions which occur in the catalogue are not such as have arisen from want of attention, but on the contrary from more than common and long inspection.

Whoever looks a long while at two stars which are equal, A and B for instance, will find that he is not always pleased with the expression A . B, but would incline rather to put them down A , B when A seems to have the preference, or A ' B when the advantage is on the side of B. Since, therefore, these three expressions A ' B A . B A , B seem equally to belong to the stars, my compound character A ; B is in that instance an useful one, which includes them all. This may seem to be a doubtful expression, but it is in fact a very positive one, amounting to A = B. For had the stars not been perfectly equal, the same causes which bring on these little waverings in the appearance of stars, whatever they are, would have operated so as perhaps to produce the comparative wavering lustres expressed by A ; B and A ≳ B or A ≲ B which denotes the union of the three expressions A . B and A , B and A – B. But if this had been the case, we could certainly not admit A = B.

Sometimes, when I was not willing to put down these compound marks, I have cast my eyes upon the ground, and after a few moments lifted them quickly up to the stars AB, and instantly decided which of the expressions ought to be used: this being repeated perhaps a dozen or more times, I took that expression for the most proper one which would occur oftener than any other in these transitory glances.

All observations upon stars of any considerable magnitude have been made with the naked eye. I was unwilling to introduce the fallacies, or at least the difficulties that occur in the use of a telescope, owing to various causes that need not be mentioned, where I could possibly do without it. In numberless instances, however, the telescope has been recurred to, notwithstanding the stars under examination were not so small but that I saw them very well with the naked eye; for in very fine nights, and in high situations, all the stars of the sixth, and most of the seventh magnitude, are sufficiently visible. But when small stars were situated very near each other, or very near brighter ones, it became necessary to remove the objection arising from the light of one star either overpowering or blending with that of the other.

Care has been taken in observations with the naked eye not to fix upon a star as a standard which has another close to it; for the united light of the two stars would certainly cause deceptions. And stars that stand in this predicament of course have been referred to others with the assistance of a telescope.

The largest stars, and in general all such as had no convenient stars in the same constellation to be compared with them, have their lustre ascertained by such as I could find in the neighbouring part of the heavens.

Whenever I use the expression of magnitude, which though not of so nice and critical distinction as would be required for the purpose of my catalogue, is still a very useful one for general purposes, I have endeavoured to conform my mental standard to the notation of FLAMSTEED.

The most remarkable expressions of brightness which are contradictory to FLAMSTEED's magnitudes, are pointed out in

the notes annexed to the constellations. They are pretty numerous, and with many stars so considerable, that we have great reason to suspect changes in their lustre since FLAMSTEED's time. It is to be noticed, that in collating my observations of brightness with FLAMSTEED's magnitudes, I have not only taken those which are in the British catalogue, but also those that are to be found in the *Observationes Fixarum*. The very extraordinary disagreement between the former and the latter ought not to pass unnoticed. Were it not for what FLAMSTEED says in his *Prolegomena*, when he mentions the arrangement of the catalogue, " Undecima columna indicat " cujus magnitudinis stellam esse arbitratus sum quando eam " observatam habui," I should entirely reject the magnitudes of the catalogue as being without authority to support them. Nor can I conceive how such a remarkable disagreement could escape the author's notice, or remain unperceived by astronomers till this time, if the lustre of the stars in general had not been looked upon as a thing of no material consequence.

To shew what the difference is to which I allude, let us cast an eye upon the 9 constellations which are contained in the following catalogue of brightness.

In Aquarius there are 108 stars. To 49 of these no magnitudes can be found in FLAMSTEED's observations; of 38 the magnitudes annexed to them agree with those of the catalogue; and of 21 they disagree with them.

In Aquila there are 71 stars. 39 are not observed; 16 agree; 16 disagree.

In Capricornus are 51 stars. 22 not observed; 17 agree; 12 disagree.

In Cygnus are 81 stars. 47 not observed; 21 agree; 13 disagree.

In Delphinus are 18 stars. 11 are not observed; 3 agree, and 4 disagree.

In Equuleus are 10 stars. 5 are not observed; 3 agree, and 2 disagree.

In Hercules are 113 stars. 10 are not observed; 54 agree, and 49 disagree.

In Pegasus are 89 stars. 22 are not observed; 37 agree, and 30 disagree.

In Sagitta are 18 stars. 3 are not observed; 13 agree, and 2 disagree.

To this may be added, that the disagreement in several stars is so considerable as to amount to two magnitudes; in many to one and an half, and in still more to one magnitude: not only with stars of a small size, but with some of the brightest in the constellation. I do not include α Cygni, which is marked 2m in the catalogue, and in the observation 7m, as that must certainly be a mistake; but cannot help regretting that a work to which every astronomer has been taught to look up as the first authority, should have been sent to the press with so many errors, that we hardly know how far to give our confidence to what is laid down in it.

I. *Catalogue of the comparative Brightness of the Stars.*

Lustre of the stars in Aquarius.			
1		6	70 Aquilæ, 1
2	ε	5·4	2 – 23 2 – – 6
3		5	3 – 5
4		6	5, 4
5		6	3 – 5, 4
6	μ	4·5	13, 6 – 7 6, 18 2 – – 6 – 7
7		6	6 – 7 – – 8 18, 7
8		6.7	7 – – 8, 9
9		6	8, 9
10		6	11, 10
11		6	12, 11, 10
12		6	12, 11
13	ν	5	23. 13, 6
14		6	17 – 14
15		6	21. 15, 16
16		6	15, 16, 20
17		6	19, 17 – 14
18		6	6, 18, 7
19		6	19, 17
20		6	16, 20
21		6	21. 15
22	β	3	34 ; 22, 49 Capricorni
23	ξ	6	2 – 23. 13
24		6	26 – 24
25	*d*	6	25. 27
26		6	27, 26 – 24
27		6	25. 27, 26
28		6	32, 28 28, 30

Lustre of the stars in Aquarius.			
29		6	35, 29
30		6	46, 30 30-60 28, 30 30--36
31	o	5	31-32
32		6	31-32, 28
33	ι	4	73, 33, 57 33. 23 Capricorni
34	α	3	34. 88 Pegasi 34; 22
35		6.5	41, 35, 29
36		6	30--36 37, 36
37		6	45. 37 37, 36
38	e	6	38, 42
39		6	42. 39, 45
40		7.8	45. 40 40, 61
41		6	47, 41, 35 41-49
42		7	38, 42, 45 42. 39 42, 53
43	θ	4	71, 43, 57
44		6	51, 44
45		6	42, 45 39, 45. 40 45. 37 45. 50
46	ϱ	5.6	46, 30
47		5.6	47, 41 59, 47. 68
48	γ	3	62. 48-52
49		5	41-49
50		6	45. 50, 56
51		6	63, 51, 44
52	π	5	48-52
53		6	42, 53
54		6	58. 54
55	ζ	4	76. 55, 62
56		6	50, 56, 61
57	σ	5	33, 57 43, 57
58		6	58. 54 74-58-64
59	υ	5	66; 59; 47
60		6	30-60
61		6	56, 61 40, 61

Lustre of the stars in Aquarius.			
62	η	4	55, 62 . 48
63	$\varkappa$	5	63, 51
64		6	58 – 64 . 65
65		6	64 . 65 . 75
66	g^1	6	66 –, 59
67		6	67 . 78
68	g^2	6	47 . 68
69	τ^1	5	7 –, 69 69 –, 77
70		6	70 . 74
71	τ^2	6 . 5	73, 71, 43 71 –, 69
72		6	Does not exist.
73	λ	4	73, 33 73, 71 73, 88
74		6	70 . 74 – 58
75		7	65 . 75
76	δ	3	76 . 55
77		6	69 –, 77
78		6	67 . 78 81, 78 80 - 78 . 80
79		2 1	8 Pegasi, 79, 44 Pegasi
80		7	80 – 78 . 80
81		7	82, 81, 78
82		7	82, 81
83	b^1	6	83, 92
84	b^2	7	87 – 84
85	b^3	6	92 – – 85, 87
86	c^1	6	18 Piscis aust . 86 99, 86 . 89
87	b^4	6	85, 87 – 84
88	c^2	4	88 – 18 Piscis aust 73, 88 –, 98
89	c^3	5 . 6	86 . 89 . 101 89, 104
90	ϕ	5	93 . 90 – 92 91, 90 . 93
91	Ψ^1	5	91, 90
92	χ	6	90 – 92, 96 83, 92 – – 85
93	Ψ^2	5	93 . 90 90 . 93
94		6	94, 95

Lustre of the stars in Aquarius.			
95	ψ^3	5	94, 95, 97
96		6.7	92, 96
97		6	95, 97
98	b^1	5	88 ; 98, 99
99	b^2	5	98, 99, 86
100	b^3	5	101 – 100
101	b^4	5	89. 101 – 100
102	ω^1	5	105, 102
103	A^1	5	104. 103. 106
104	A^2		89, 104. 103
105	ω^2	5	105, 102
106	A^3	5	103. 106. 107
107	A^4	6	106. 107. 108
108	A^5	6	107. 108

Lustre of the stars in Aquila.			
1	*m*	4	16, 1, 12
2	*o*	5	2. 3
3	*n*	5	2. 3. 9
4		5	9, 4, 5
5		6	4, 5
6	*l*	4	6. 12 6. 63 Serpentis
7		6	8, 7
8			8, 7
9	*k*	5.4	3, 9 12, 9, 14 9, 4
10		6	11, 10
11		6	18 ; 11, 10
12	*i*	4	1, 12 6. 12, 9 12. 63 Serpentis
13	ε	3.4	13 – 18
14	*g*	6	9, 14, 15
15	*b*	6	14. 15
16	λ	3	16, 30 16, 65 15, 1
17	ζ	3	50, 17 – 65

Lustre of the stars in Aquila.						
18		6	13 – 18, 11	18, 19	18 – 11	
19		6	18, 19, 22	21; 19		
20		5.6	26 . 20	37 . 20		
21		5	21; 19	23 . 21		
22		6	19, 22			
23		7	23 . 24	23 . 21	23 – 24	
24		7	23 . 24	23 – 24		
25	ω^1	6	28 . 25	25 –, 29		
26	*f*	6	39, 26 . 20			
27	*d*	6	32 – 27	27 – 35		
28	A	6	31, 28 . 25			
29	ω^2	7	25 –, 29			
30	δ	3	65, 30, 55	16, 30	65; 30	
31	*b*	6	31, 28			
32	ν	5	41 – 32 – 27	38, 32, 44		
33		6	Does not exist.			
34		6	Does not exist.			
35	*c*	6	27 – 35			
36	*e*	6	36, 42	36 – 45		
37	*k*	6	37 . 20	39, 37, 51		
38	μ	4	41 – 38 . 44	38, 32	38 ; 59	67; 38
39	$\varkappa$	3.4	39, 26	39, 37		
40		6	Does not exist.			
41	ι	3.4	55 – 41 – 38	41 – 32	41 ; 71	41, 55
42		6	36, 42, 45	62, 42 . 66	58, 42	
43		6	Does not exist.			
44	σ	5	38 . 44	32, 44	59, 44 . 54	
45		6	42, 45	36 – 45		
46		6	61 . 46	47, 46, 48		
47	χ	6	47 . 52	47, 46		
48	ψ	6	46, 48			
49	υ	6	63 . 49			
50	γ	3	53 – – – 50, 17	50, 34 Sagittarii		

Lustre of the stars in Aquila.			
51		5	37, 51 56. 51
52	π	6	47. 52. 61
53	α	1. 2	53 – – – 50 21 Scorpii – – 53 – – 50 Cygni
54	o	6. 5	44. 54 54, 63 59 – 54
55	η	3. 4	30. 55. 60 55 – 41 41, 55
56		5	57 – – 56. 51
57		6	57 – – 56
58		6	62, 58, 42 58 – 66
59	ξ	5	38 ; 59, 44 59 – 54
60	β	3. 4	30, 60. 55
61	φ	6	52. 61, 46
62		6	62, 58 66. 62, 64
63	τ	6	54, 63. 49
64		6	66, 64 62, 64
65	θ	3	17 – 65, 30 16, 65 65 ; 30
66		5. 6	42. 66 66, 64 66. 62 58 – 66
67	ϱ	5	67 ; 38
68		6	69, 68 69 – 68
69		5	70, 69, 68 70, 69 – 68
70		5	71 – 70, 69 70, 1 Aquarii
71		4	71 – 70 41 ; 71
Lustre of the stars in Capricornus.			
1		6	2, 1. 3
2	ξ	6	2, 1
3		6	1. 3
4		6	7, 4
5	α^1	4	6 – 5 –, 8
6	α^2	3	6 – 5
7	σ	obs.	10, 7, 12 15. 7, 4
8	ν	6	5 –, 8, 11
9	β	3	49 – 9 – – 40
10	π	obs.	11, 10, 12 10, 7 10, 15
11	ϱ	6	8, 11, 10

Lustre of the stars in Capricornus.			
12	*o*	obs.	10, 12 7, 12 15, 12
13		6	15–13 14 =; 13
14	*τ*	6	14, 15 14 =; 13
15	*ν*	6	10, 15, 12 15.7 14.15–13
16	*ψ*	5	16, 18
17		6	24; 17
18	*ω*	6	16, 18; 24
19		6.7	23--19, 21
20		6.7	20.21 22–20.25
21		6	19, 21 20.21
22	*η*	5	23–, 22–20 22–24
23	*θ*	5	23--19 23–, 22 33 Aquarii . 23
24	A	6	18; 24; 17 22–24, 25
25	χ^1	6	20.25–, 26 24, 25
26	χ^2	6	25–, 26.27
27	χ^3	6	26.27
28	*φ*	6	36–28, 33
29		6	32–29–30
30		6	32--30–31 29–30
31		7	30–31
32	*ι*	5	32--30 32–29
33		6	36, 33, 35 41, 33 28, 33, 35 34 Sagittarii . 33
34	*ζ*	5	34, 39 34--36
35		6	33, 35
36	*b*	6	39–36, 43 36, 33 34--36–28
37		6	43–, 37, 38
38		6	37, 38
39	*ε*	4	34, 39–36
40	*γ*	4	9--40
41		6	41, 33
42	*d*'	6	42–44 42–, 44 42; 51 42.48 51; 42; 48

Lustre of the stars in Capricornus.			
43	$\varkappa$	5	36 , 43 –, 37
44	d^2	6	42 – 44 · 45 42 –, 44 , 45 44 ; 45
45		6	44 ; 45 44 , 45 44 · 45
46	c^1	6	46 –, 47
47	c^2	6	46 –, 47
48	λ	5	42 · 48 , 51 48 – – 50 42 ⁻ ; 48
49	δ	3	49 – 9 22 Aquarii , 49
50		6	48 – – 50
51	μ	5	42 ; 51 48 , 51 51 ; 42
Lustre of the stars in Cygnus.			
1	$\varkappa$	4	1 . 10 1 ⁻ ; 10 10 ⁻ ; 1 10 . 1 1 – 32
2		5	12 ; 2 ⁻ ; 9
3		6	8 Vulpeculæ . 3 3 Vulpeculæ – – 3
4		6	14 · 4 12 , 4 15 , 4 – 11 8 – 4 , 11
5			Does not exist. A small star near the place 9 – – 5
6	β	3·4	53 , 6 , 18 18 ; 6 53 ⁻ ; 6 6 –. 14 Lyræ
7		6	16 , 7
8		6	17 . 8 8 ; 15 8 , 12 8 , 14 8 – 4 21 – 8 – 17
9			12 ⁻ ; 9 12 – 9
10	ι	6	1 . 10 10 . 1 10 ⁻ ; 1 65 ⁻ ; 10
11		6	4 , 11
12	φ	5	12 ; 2 8 , 12 , 4 12 – 9
13	θ	4	32 , 13 33 , 13 , 20 33 , 13 , 23
14		6	8 , 14 · 4
15		6	15 , 22 8 ; 15 , 4 15 . 25
16	c^1	6	16 , 7
17	χ	5	8 – 17 21 – – 17 . 8 21 = ⁻ ; 17
18	δ	3·4	6 , 18 , 64 53 , 18 53 –, 18 – 64 53 , 18 , 64 8 ; 6 18 ⁻ ; 64
19		6	22 , 19 25 , 19

Lustre of the stars in Cygnus.			
20		5.6	24 . 20 . 26 13 , 20
21	η	6	21 . 41 21 – – 17 21 = ; 17 21 – 8
22		6	25 . 22 22 , 25 15 , 22 , 19
23		6	13 , 23
24	ψ	5	24 . 20
25		6	15 , 25 . 22 22 . 25 , 19
26	c^2	6	20 . 26
27	b^1	5	36 – 27
28	b^2	5	34 ; 28 , 36 28 . 35 28 . 34
29	b^3	6	29 . 34 34 . 29
30	o^1	4	32 – 30 32 –, 30
31	o^2	5	31 , 32
32		5.6	32 . 33 32 , 13 31 , 32 – 30 1 – 32 31 – 32 –, 30
33			32 . 33 , 13
34		6	29 . 34 ; 28 28 . 34 34 . 29 34 , 36 34 , 40
35	m	6	28 35 39 – 35
36		6	28 , 36 – 27 34 . 36
37	γ	3	37 , 53 37 – 53 37 –, 53 37 ; 8 Peg. 37 – 8 Peg. 37 ; 8 Pegasi 5 Cephei –, 37
38			Does not exist, or is lost.
39	b	6	41 – 39 – 35 47 – 39 – 35
40		6	34 , 40 40 , 42
41	i	4	41 , 52 21 . 41 41 – 39
42		6	40 . 42 , 44
43	w^1	5	46 . 43
44		6	42 , 44
45	w^2	5	45 , 46
46	w^3	5	45 , 46 , 43
47	l	6	47 – 39
48		6	49 – 48 ; 48 49 –, 48 ; 48 49 – – 48 . 48
49		6	49 – 48 ; 48 . 49 –, 48 : 48 49 – – 48 , 48

Lustre of the stars in Cygnus.						
50	α	2	50 - - - 37	53 Aquilæ ; 50	53 Aquilæ - - 50	
51		6	56 , 51			
52	*k*	6	41 , 52			
53	ε	3	37 , 53 , 6	37 - 53 , 18	37 - 53 -, 18	37 -, 53 ; 6
54	λ	4	67 . 54			
55		6	56 . 55 - 59	55 . 63	63 . 55 , 59	
56		6	57 , 56 . 55	57 , 56 , 51		
57		6	57 , 56			
58	ν	4	62 . 58 . 67			
59	f^1	5.6	55 - 59 , 60	63 , 59	55 , 59	59 , 68
60		6	59 , 60			
61		6	70 . 61 , 69			
62	ξ	4	65 , 62 , 58			
63	f^2	6	55 . 63 , 59	63 . 55	68 , 63	
64	ζ	3	18 , 64	18 ; 64		
65	τ	4	65 , 62	65 - 66	65 ; 10	
66	υ	5	66 , 78	65 - 66 , 67		
67	σ	4	58 . 67 . 54	67 ; 78	66 , 67	
68	A	6	59 , 68 , 63			
69		6	69 . 70	70 - 69	79 , 69	61 , 69
70		6	69 . 70	72 , 70 - 69	70 . 61	
71	*g*	6	80 . 71			
72		6	72 , 70	74 , 72		
73	ϱ	4	73 - 81			
74		6	74 , 72	74 - 77	75 . 74 , 72	
75		6	75 . 74			
76		6	77 , 76			
77		6	74 - 77 , 76			
78		3 . 4	66 , 78	67 ; 78 - 14 Pegasi		
79		6	79 , 69			
80	π^1	4	81 - 80 . 71			
81	π^2	5	73 - 81 - 80			

Lustre of the stars in Delphinus.				
1		6	8, 1, 10	
2	ε	3	12, 2, 11	
3	η	6	5·3, 8	
4	ζ	5	11–4·7	
5		6	7, 5·3	
6	β	3	6, 9	
7		6	4·7, 5	
8	θ	6	3, 8, 1	
9	α	3	6, 9·12	
10		6	1, 10	
11	δ	3·4	2, 11–4	
12	γ	3	9·12, 2	
13		5	13–14	1 Equulei – 13
14		6	13–14	
15		6	18, 15	
16		6	17, 16, 18	
17		6	17, 16	
18		6	16, 18, 15	

Lustre of the stars in Equuleus.				
1		5	10, 1–13 Delphini	
2		6	4, 2	
3		6	3·4	
4		6	3·4, 2	
5	γ	4	8--5·7	5–6
6		6	5–6	
7	δ	4	5·7, 10	
8	α	4	8--5	
9		6	10–9	
10	β	4	7, 10, 1	10–9

Lustre of the stars in Hercules.			
1	χ	6	1–, 4 1, 30 30 . 1
2		6	4, 2 –, 14
3		5	3 – 9
4		6	1–, 4, 2
5	r	5	7 – – 5 – 16
6	υ	6	35, 6 – – 14 35 – 6, 30 6, 52 11, 6 11 ; 6
7	$\varkappa$	5	7 – – 5
8	q	5.6	16, 8
9		6	3 – 9, 43 Serpentis
10		5	10 – 17 10 – 19
11	φ	6	11 . 35 – 6 11 – – – 14 35 . 11, 6 11 ; 6 22, 11
12		6	21 – – 12 – 15
13		5.6	15 . 13
14		7	6 – – 14 11 – – – 14 2 –, 14
15		6	12 – 15 . 13
16		6	5 – 16, 8
17		6	10 – 17, 18 19, 17, 18
18		7	17, 18
19		6	10 – 19, 17
20	γ	3	64 –. 20 22, 20 . 58 22 – 20
21	o	6	21 – – 12 28 – 21
22	τ	4	44, 22, 20 22, 8 44 – 22 – 20 22, 11
23		5	20 Coronæ – 23 23 – 26
24	w	6	24 ; 29 24, 60 24 – 29 24 –, 29
25		5	30 – 25 30 –, 25 59, 25
26		7.6	23 – 25 25 – 31
27	β	3	27 –, 64 40 – 27 27 ; 40 27 – – 64 27 ; 40 27, 40
28		6	28 – 21
29	b	4	24 ; 29 60 ; 29 24 – 29 24 –, 29

Lustre of the stars in Hercules.			
30	*g*	5	1, 30 30–25 6, 30 52, 30 42, 30, 34 30. 1 30–, 25
31		7	26–31
32		6	48, 32
33		6	41, 33
34		6	30, 34
35	σ	4	58, 35 85, 35 11, 35, 6 35. 11
36	*m*	6	37 – – 36 38 – 36
37	*m*	6	37 – – 36 37 – 38 37, 45
38		6	37 – 38 41, 38 – 36
39		5	39 – 50
40	ξ	3	40 – 27 27; 40 27; 40 27, 40 27; 40 37 Serpentis – 40 –, 64
41		6	45 – – 41 47, 41, 38 41, 33
42		5	52, 42, 30
43	*i*	5. 6	45, 43, 47
44	η	3	65, 44, 22 44, 86 44 – 22 65 – 44 – 58
45	*e*	5	45 – – 41 37, 45, 43
46		7	48 – 46
47	*k*	5	43, 47, 41
48		6	50 – 48, 32 48 - 46
49		6	60 – – 49
50		5	39 – 50 – 48 53 – – 50
51		5	53 · 51 – – 56
52		5. 6	6, 52, 30 52, 42
53		5	53 – – 50 53 · 51
54		5	One of these two does not exist.
55		5	60, 54 or 55
56		6	51 – – 56 · 57
57		6	56 · 57
58	ϵ	3	20 · 58, 35 58, 91 44 – 58 103 – 58 – 76
59	*a*	6	68, 59, 61 59, 25

Lustre of the stars in Hercules.			
60		6	24, 6 ; 29 60 – – 49 60, 66 60, 54 or 55
61	*c*	6	59, 61
62		6	71 – – 62
63		6	72 . 63 63, 78
64	α	3	64 – 27 Ophiu 27 Ophiu – 64 64, 65 65 –, 64 27 –, 64 –, 20 64 ; 67
65	δ	4	65 – 64 67, 65 64, 65 – 44
66	ω	6	60, 66, 37 Ophiuchi
67	π	3·4	67, 65 67, 27 Ophiuchi
68	*u*	5	70 . 68, 72 68, 90 68, 59 69 – 68
69	*e*	4·5	76 – 69 69 – 68 94, 69 – 99
70		4	70 – – 62 70 . 68 70 – 73
71		5	Does not exist.
72	*w*	6	68, 72 . 63 90, 72
73		6	70 – 73
74		6	77, 74 . 88
75	ϱ	4	75, 76 91, 75 75, 94
76	λ	4·5	75, 76 – 69 58 – 76
77	*x*	6	82 . 77, 74
78		6·7	63, 78 78, 93
79		6	79 – 83 89, 79
80		4	Does not exist.
81		4	Does not exist.
82	*y*	6	82 . 77
83		7	79 – 83 . 84
84		7	83 . 84
85	ι	4	22, 85, 35 14 Lyræ, 85
86	μ	4	44, 86, 92 86 – 92
87		6	87 – 89
88	Z	6	74 . 88
89		6	87 – 89, 79
90	*f*	6	68, 90, 72

Lustre of the stars in Hercules.			
91	θ	4	58, 91, 75 92 . 91
92	ξ	4	86, 92 . 103 103 . 92 . 91 92 . 109 92, 109 86 – 92 ; 103
93		5	78, 93
94	ν	5	75, 94, 69 103 – 94 . 100 109, 94
95		4	102, 95, 101 95 . 98
96		5	101, 96 . 97 101 . 96 – 98
97		5.6	96 . 97 98 . 97
98		5	95 . 98 96 – 98, 97
99	*b*	5	69 – 99 – 104 106 . 99 100, 99 107, 99 . 108
100	*i*	6	94 . 100, 99
101		5	95, 101 . 96 95, 101 . 96
102		4.5	102, 95
103	*o*	4	109 . 103 92 . 103 – 94 92 ; 103 – 58
104	A	4.5	99 – 104
105		5	106 – 105
106		5.6	106 . 99
107	*t*	6	107, 99
108		6	99 . 108
109		4	92 . 109 . 103 92, 109, 94 109, 111
110		4.5	111, 110, 113
111		4	109, 111, 110
112		5	113, 112
113		5	110, 113, 112
Lustre of the stars in Pegasus.			
1	*e*	4	24 – 1, 10 1 . 9
2	*f*	4.5	10 – 2, 16 9 – 2 . 13 2 – 13
3		6	3, 4
4		6	3, 4 . 7
5		6.7	13 – 5 12, 5 13, 5
6		6	16, 6 – 11

Lustre of the stars in Pegasus.			
7		6	4 · 7
8	ε	3	8 – 54 37 Cygni – 8 21 Andr ; 8 , 54 8 , 53 8 ; 16 Ceti 8 , 24 Pisc austr
9	g	4 · 5	1 . 9 – 2
10	κ	4	1 , 10 – 2
11		6	6 – 11
12		6	16 , 12 , 5
13		6	2 . 13 – 5 13 , 17 13 , 5 2 – 13
14		6	16 , 14 , 15 78 Cygni – 14
15		6	14 , 15
16		6	2 , 16 16 , 6 16 , 12 16 , 4
17		6	13 , 17 . 21 17 , 21
18		5	22 – 18 . 19 19 , 18
19		6	18 . 19 19 , 18 22 – 19
20		6	21 , 20
21		5	17 . 21 , 20 17 , 21 , 20
22	ν	5	46 , 22 – 18 22 , 35 22 – 19 31 , 22
23		6	38 , 23
24	ι	4	24 – 1
25		6 . 7	25 – 28
26	θ	4	42 , 26 – 46
27		5	29 – 27
28		6 . 7	25 – 28
29	π	4 · 5	29 – 27
30		6	35 , 30 31 , 30 – 36 31 =' 30
31		4 · 5	31 , 30 31 =' 30 31 , 22 50 · 31 , 49
32		6	43 , 32 , 38 43 · 32
33		6 . 7	39 · 33
34		6	37 , 34
35		6	22 , 35 , 37 35 , 30
36		6 . 7	30 – 36
37		6	35 , 37 , 34
38		6	32 , 38 , 23

Lustre of the stars in Pegasus.						
39		6.7	41, 39, 45	39 · 33		
40		6	40, 41			
41		6.7	40, 41, 39			
42	ζ	3	48, 42, 26			
43	o	5	47 – 43, 32	56, 43, 32		
44	η	3	53 ; 44 ; 48	53, 44, 88	54, 44; 88	
			16 Ceti ; 44			
45		6.7	39, 45			
46	ξ	5	26 – 46, 22	46 – 50		
47	λ	4	48, 47 – 43	47, 68	47 –, 68	48, 47 – 68
48	μ	4	44 ; 48, 47	88 – – 48, 42		
49	σ	6	50, 49, 52	31, 49		
50	ϱ	6	46 – 50, 49	50 · 31		
51		6	56 – 51, 60			
52		6	49, 52			
53	β	2	54, 53 ; 44	54 · 53, 44	8, 53 · 54	
54	α	2	8 – 54	54, 53	8, 54 · 53	53 · 54, 44
55	l	5	55, 59			
56		5.6	62 – 56 – 51	56, 64	56 · 72	56, 71
			62 – 56	56 · 43	78; 56	
57	m	6	58, 57			
58	n	6	59, 58, 57			
59	p	6.5	55, 59, 58			
60		6	51, 60, 61			
61		6	60, 61			
62	τ	6	68, 62 – 56	62, 78	84, 62	62 – 56
63		6	67 – 63 · 73			
64		6	56, 64 · 67			
65		6	69, 65			
66		6	70 – 66, 86			
67		6.7	64 · 67 – 63			
68	υ	6	47, 68, 62	68, 70	47 –, 68	47 – 68
69		6	71, 69, 65			

Lustre of the stars in Pegasus.			
70	*q*	5 6	68 , 70 – 66 70, 82
71	*y*	6	56 , 71 , 69 71 , 85
72		6	56 . 72
73		6	63 . 73
74		7	75 , 74 , 76 75 ; 74 , 76
75	S	6	81 , 75 , 74 75 ; 74
76		6	74 , 76
77		6	82 , 77 – 80
78		5 . 6	62 , 78 – 79 62 , 78 ; 56
79		6	78 – 79
80		6	77 – 80
81	φ	6	89 , 81 , 75
82		6	70 82 , 77
83	*r*	6	87 , 83 23 Piscium – 83
84	ψ	6	84 . 62 84 , 89
85		6	71 , 85 87 . 85
86		5 . 6	66 , 86
87	*u*	6	87 . 85 87 , 83
88	γ	2	44 , 88 44 ; 88 – – 48 34 Aquarii . 88 88 ; 6 Arietis
89	χ	6	84 . 89 , 81
Lustre of the stars in Sagitta.			
1		6	1 Vulpeculæ – 1 2 Vulpeculæ , 1
2		6	2 . 3
3		6	2 , 3
4	ε	5	6 –, 4
5	α	4	5 . 6 7 –, 5 . 6 5 – 9 Vulpeculæ
6	β	4	5 . 6 –, 4 5 . 6 , 8
7	δ	4 . 5	7 –, 5 12 – 7
8	ζ	6	6 , 8 – 9 8 16 9 Vulpec , 8 12 Vulp , 8
9		6	8 – 9

Lustre of the stars in Sagitta.			
10		6	11, 10 . 15
11		6	11, 10
12	γ	4	12 – 7
13	χ	6	15 . 13
14	y	6	14, 15
15	Z	6	14, 15 . 13 10 . 15
16	η	6	8 . 16 –, 17
17	θ	6	16 –, 17 – 18
18		6	17 – 18

Notes to Aquarius.

2 "August 2, 1788. 20-feet reflector 2 (ε) 4.3m FL. 5.4 m." The difference amounts to one whole magnitude. In FLAMSTEED's observations no magnitude is mentioned.

6 Is less than 13, and very little brighter than 18. The former is contrary to the catalogue, and the latter inconsistent with the magnitude assigned to 18. None of these stars have any magnitude in FLAMSTEED's observations.

8 Is larger than 9, contrary to the catalogue. In the observations 8 is 6m, but 9 has no magnitude.

13 Is less than 23, and is larger than 6; both are contrary to the catalogue. There are no magnitudes of either of these stars in FLAMSTEED's observations.

23 Is larger than 13, contrary to the catalogue, and from the expression 2 – 23 (see 2) it appears that 23 is undervalued by FLAMSTEED, or has changed its lustre. FLAMSTEED's observations give no magnitude of 23.

34 Is equal to 88 Pegasi, which the catalogue has 2m. See 88 Pegasi.

35 Is less than 41, contrary to the catalogue, "Oct. 13, 1786, 5 . 6m." There is no magnitude of 35 in FLAMSTEED's observations.

40 Is larger than 61, contrary to the catalogue. This is a considerable deviation, amounting to $1\frac{1}{2}$m. In the observations 40 is 7m, 61 6m.

41 Is larger than 49 and 35, contrary to the catalogue. It is also contrary to the observations. "Oct. 13, 1786, 41 6 . 5m."

42 Is larger than 45, 39 and 53, contrary to the catalogue. The observations give 6m to 53.

43 Is less than 71. See 71. There is no magnitude to either of these stars in FLAMSTEED's observations.

48 Is less than 62, contrary to the catalogue; and is now probably less bright than it was formerly. 48 being but little brighter than 52 confirms the same. There is no observation of 48, but 62 is 5m.

59 Is less bright than 66, contrary to the catalogue. The observations give 59 6m.

71 Is brighter than 69, contrary to the catalogue. These stars are so near each other that a change must be evident, unless FLAMSTEED should have made a mistake in writing down their magnitudes. 71, 43 confirms the same conclusion. In the observations neither 69 nor 71 has a magnitude assigned.

72. There is no observation of FLAMSTEED upon this star.

78 Is less than 81, contrary to the catalogue; and in the observations it is 5m.

79 Is less than 8 Pegasi, contrary to the catalogue. The difference between 2.1m and 3m would be striking, if the lowness of the situation of 79 did not render its real magnitude very uncertain. In my estimation no allowance is made for that low situation. In the observations there is no magnitude to either of these stars.

80 There are two stars, the smallest of which agrees best with the place of 80 in Atlas, but neither of them seems to accord completely in relative situation with 81 and 82. In one of my sweeps a star, supposed to be 80, was taken with the following deduction; "Sept. 12, 1785. This star requires a correction of — 1′ 13″ in time of RA, and — 6′ in PD."

84 Is larger than 87, contrary to the catalogue. In the observations they are both 8m.

85 Is much less than 92, which does not agree with the magnitudes of the catalogue. In the observations it is marked 8m.

86 Is larger than 89, contrary to the catalogue. There is no magnitude to either of these stars in the observations.

88. "Oct. 13, 1786, 20-feet reflector, 4.3m." FLAMSTEED'S observations give it 4m.

89 Is larger than 101 and 104, contrary to the catalogue. In the observations 104 is 6m.

94 Is larger than 95, contrary to the catalogue. In the observations they are both 5m.

96. "Sept. 12, 1785, 6m." In FLAMSTEED'S observations it is also marked 6m.

Catalogue.

80 Requires — 18′ in RA, and — 6′ in PD.
The PD of 96 requires — 8′.

Atlas.

The RA of 30 requires + 1°.
72 must be out.
80 requires — 18′ in RA, and — 6′ in PD.
84 requires — 10′ in RA, and — 12′ in PD.
85 requires — 28′ in RA.
96 requires — 8′ in PD.

Notes to Aquila.

" July 23, 1781. Order of magnitude $\alpha\,\gamma\,\zeta\,\theta\,\delta\,\eta\,\beta\,\varepsilon$."

3 Is larger than 9, contrary to the catalogue. In FLAMSTEED's observations both are marked 6m.

6 and 12 are both larger than 63 Serpentis; but that star is placed among the changeable ones. See Phil. Trans. Vol. LXXVI. page 211. FLAMSTEED's observations give 3m. to 6.

13. " Sept. 3, 1784; 20-feet reflector, 13 (ε) 5.6 FL. 3.4m, but strong twilight." It is not much larger than either 11, 18, or 19, so that we may be pretty certain it must have lost some of its lustre since the time of FLAMSTEED. In his observations it is marked 4m.

20 Is less than 26 and 37, contrary to the catalogue. In the observations 20 is marked 5m.

21 Is less than 23, contrary to the catalogue. But in the observations 23 is marked 5m. The error therefore is probably in the catalogue.

24 The star I estimate is one of two small ones.

33 There is no observation of this star in FLAMSTEED's work.

34 This star was never observed by FLAMSTEED.

37 Is larger than 20 and 51, contrary to the catalogue. The latter is marked 6m in the observations.

38 Is less than 67, contrary to the catalogue.

39 Is not much larger than 26 and 37, which will not agree with 3.4m of the catalogue; but in FLAMSTEED's observations it is put down only 5m.

40 There is no observation of this star in FLAMSTEED's work.

43 There is no observation of this star in FLAMSTEED's work.

55 This star is periodical. The time of its period as given by Mr. PIGOTT, the discoverer, is $7^d\ 4^h\ 15'$. See Phil. Trans. Vol. LXXV. page 127.

56 Is much less than 57, contrary to the catalogue; but in the observations 56 is only marked 6m.

66 Is less than 42 and 58, contrary to the catalogue, and it is moreover marked 5m in the observations.

Atlas.

The RA of 23 requires — 20′.

29 Should be 30′ from 25, and 48′ from 28, on the south following side of the two stars.

The stars 33, 34, 40, and 43 should be out.

Notes to Capricornus.

"Sept. 27, 1782. Order of magnitude $\delta \beta \alpha \gamma$."

6 in 1780 was less than $\beta \gamma \delta \zeta \eta \theta \iota$.

13 Is not equal to 14 as the catalogue gives it. In FLAMSTEED's observations 14 is without magnitude assigned to it.

19 and 20 are larger than 21, contrary to the catalogue. In the observations 19 is marked 6m.

34 Is larger than 39, contrary to the catalogue. Neither of them has any magnitude given with them in FLAMSTEED's observations.

36 Is larger than 43, contrary to the catalogue. It has either been under-rated, or gained additional lustre since FLAMSTEED's time. Neither of the stars has any magnitude in his observations.

42 Is larger than 48, contrary to the catalogue. The latter has no magnitude in the observations, and the former is marked once of the 5th and once of the 6th, which may be put down 5 . 6m.

Catalogue.

The letter *e* should be added to 26. FLAMSTEED has used it in his observations, page 75.

Atlas.

31 requires about + 22′ in RA.

Notes to Cygnus.

"May 12, 1783. Order of magnitude $\alpha \quad \gamma \varepsilon \beta \delta \quad \zeta \quad \theta$."

5. There is no observation of this star by FLAMSTEED.

Page 67 a star was observed without time, but by page 71 and 122 it appears that the defective observation belongs to 2. There is a star 8 or 9m, about 50′ from 2, 1° 20′ from 9, and and 1° 30′ from 6; and calling that star 5, its brightness may be expressed by 9 – – 5.

10. "Sept. 15, 1783. 10 is at least 4m. It is larger than 13." If the authority of the catalogue be good, there can be no doubt of a change since FLAMSTEED's time; but in his observations there is no magnitude to this star.

12 Is less than 8, contrary to the catalogue. "Sept. 7, 1784, 12 (φ) 6m." In FLAMSTEED's observations there is no magnitude to either of the stars.

13 Is less than 32, contrary to the catalogue. But in the observations 13 has no magnitude.

17 Is less than 21, contrary to the catalogue. But in the observations neither of the stars have any magnitude.

18 Is larger than 64, contrary to the catalogue. But in FLAMSTEED's observations neither of the stars have any magnitude.

21 Is larger than 41, contrary to the catalogue. But 21 is without magnitude in the observations.

23. The expression 13, 23 does not agree with the catalogue. But 13 has no magnitude in the observations.

27 Is less than 36, contrary to the catalogue; but in FLAMSTEED's observations are no magnitudes of these stars.

30 Is less than 32, contrary to the catalogue; it is also contrary to the observations, which give 30 5m and 32 6 . 5m.

31 Is larger than 30, contrary to the catalogue. It is also contrary to the magnitudes given in the observations "Sept.

27, 1788; 20-feet reflector 30 (1st.o) 5m, FL. 4m. 31 (2d. o) 4m, FL. 5m."

34 Is a changeable star. Its period perhaps is about 18 years. See Phil. Trans. Vol. LXXVI. page 201.

38. In FLAMSTEED's observations, page 75, a star was taken without RA, marked "quæ præcedit ω." The time of this observation however is sufficiently determined by the 37 before it, and 45 and 46 just after; but there is no star visible in the space pointed out that can possibly be taken for 38. "Sept. 22, 1783, 38 lost. There is not a star of the 7, 8, 9, or 10th magnitude near the place." It therefore does not exist, or rather is lost.

41 Is less than 21, and not much larger than 52, which is contrary to the catalogue. "It is less than 4m." In FLAMSTEED's observations it is marked 4m, but 21 and 52 are without magnitudes.

48. "Sept. 5, 1784; I could not see this star, but instead of it found in the neighbourhood 2 stars of the 7th magnitude within 5 or 6′ of each other." "Nov. 15, 1795. If one of the stars be 48, its magnitude is over-rated, and must be about 7.8m. That of the two which is nearest to 49 is the largest."

59 Is less than 55, 56 and 63, contrary to the catalogue. It is also contrary to the magnitudes given in the observations: 63 is without magnitude.

66 Is larger than 78, contrary to the catalogue. See 78. "Sept. 13, 1784; 20-feet reflector 66 (v) 4m, FL. 5m. It is larger than 54 (λ), contrary to the catalogue." Neither 66 nor 54 have any magnitude in FLAMSTEED's observations.

71 Is equal to 80, contrary to the catalogue. Neither of them has any magnitude given with them in the observations.

78 Is less than 66 and 67. "It is much too small for 3.4m." In FLAMSTEED'S observations I find it marked 6m.

81 Is larger than 80, contrary to the catalogue. But in the observations there is no magnitude to either of the stars. "Sept. 27, 1788; 20-feet reflector, 81 ($2^d \pi$) 3.4m, FL. 5m." It is either undervalued in the catalogue, or grown brighter since FLAMSTEED'S time.

P. The changeable star in the neck of the swan. Its period is 396 days 21 hours. See Phil. Trans. Vol. LXXVI. page 200. Its present lustre is 17--P.

Atlas.

14 requires + 1° in PD.

5 should be out.

Notes to Delphinus.

"Aug. 14, 1781. Order of magnitude β α γ ε δ"

9. In the catalogue it is marked 3m; in FLAMSTEED'S observations it is 6m. My expression 6, 9: 12 agrees best with the catalogue.

13 "Aug. 7, 1785, 6m." In FLAMSTEED'S observations it is also marked 6m.

Atlas.

12 Should be placed about 52′ more south on plate 23. It is right on plate 25.

Notes to Equuleus.

"Aug. 13, 1781. Order of magnitude α γ δ β"

6. In the catalogue we have 4m; in the observations FLAMSTEED has once marked it 6m, and once 8m. If there

be any accuracy in these various notations, the star must certainly be changeable.

Notes to Hercules.

"May 12, 1783. Order of magnitude $\beta\,\zeta\,\alpha\quad\delta\,\eta\,\pi\quad\gamma\,\varepsilon\,\mu$."

5 Is much less than 7. My edition has this star 3m; that of 1712 has it 5m. FLAMSTEED's observations give 6m, which agrees best with 7 - - 5 as I give its present lustre.

8 Is less than 16, contrary to the catalogue. But in the observations this star has been marked twice 7m, twice 6m, and once 5m.

11 Is larger than 6 and 35, contrary to the catalogue. But in the observations we have this star given twice 4m, and once 3m. It is therefore undervalued in the catalogue, or is subject to changes in its lustre.

13 Is less than 15, contrary to the catalogue. The observations give them both 6m. "May 25, 1795, 13 and 15 are both smaller than FL. gives them, and are about 7.8m."

20 Is less than 22, contrary to the catalogue. In the observations they are both 4m.

22 "May 12, 1787. 22 (τ) 3m, FL. 4m."

23 Is not much larger than 26, contrary to the catalogue. The observations give 23 6m, and 26 7m.

25. "May 16, 1787. 25 7.6m, FL. 5m." In the observations this star is also 5m.

27. By my observations the light of this star seems to be subject to change. FLAMSTEED's observations give it twice 3m, and once 2m.

29 Is less than 24 and 60, contrary to the catalogue. In the observations 24 is marked 6 and 5m; 60 is given 5m, 6m, and

4m; and 29 is put down five times 5m, once 6m, and once 4m. Very possibly this star may be changeable.

30 Is larger than 1 and 52, contrary to the catalogue. In the observations 30 is given three times 5m; 1 twice 5m, and twice 4m; and 52 twice 5m.

37 Is larger than 45, contrary to the catalogue. But in the observations we have 37 twice 6m, once 5m; and 45 twice 6m, and twice 5m.

40. From the expressions I have given of the brightness of this star, we have great reason to suppose it to be changeable. FLAMSTEED'S observations give it 3m.

47 Is less than 43, contrary to the catalogue. The observations, however, give 47 three times 6m, and only once 5m.

52 Is larger than 42, contrary to the catalogue. In the observations both are twice marked 5m.

54 or 55. FLAMSTEED observed but one of these stars, once 4m, once 5m, and once 6m.

58 Is less than 103, and not much larger than 76, contrary to the catalogue. It is also contrary to the magnitudes of the observations.

62 "Is less than it is marked. I suppose it to be 7 or 7.8m." FLAMSTEED'S observations give it 6m.

64. From my expression of brightness it appears that this star is changeable, and I may venture to announce it periodical. A series of observations upon it will be given when the period of the changes shall have been more fully ascertained. FLAMSTEED has but one observation of its magnitude, which is 5m.

65. This star is probably changeable, but its connected re-

ference to neighbouring changeable stars has hitherto rendered it difficult to come at the truth. In FLAMSTEED's observations it is three times 3m, and twice 4m.

67. This star is probably changeable. FLAMSTEED's observations give it twice 3m.

69 Is less than 94, contrary to the catalogue; but the magnitudes in the observations are favourable to my notation.

78 Is larger than 93, contrary to the catalogue. The latter has no magnitude in the observations, and the former is marked 6m.

95 Is less than 102, contrary to the catalogue. The observations give 95 twice 4m, and 102 once 4m, and once 5m.

99 Is less than 100, 106 and 107, contrary to the catalogue; and also to the magnitudes of the observations. It is larger than 104, which is doubly inconsistent with the catalogue, and yet the observations also give to 104 a larger magnitude.

105 Is less than 106, contrary to the catalogue. The observations give once 3m, and once 6m. "July 17, 1785; 20-feet reflector, 105 7.6m FL. 5m is visibly less than 106."

Catalogue.

In the edition of 1725, 5 (*r*) should be 5m.

Atlas.

The PD of 2 requires + 34′.

The RA of 4 requires + 16′ and the PD — 34′.

The RA of 110, 111, 112 and 113 requires + 5°.

55, 71, 80 and 81 should be out.

Notes to Pegasus.

2. The expressions 2, 16 and 2.13 shew that this star is over-rated in the catalogue. In FLAMSTEED'S observations it stands 6m.

8 Is larger than 53 and 54, contrary to the catalogue. In the observations are no magnitudes of these stars.

18 Is not sufficiently distinguished from 19 to agree with the magnitudes of the catalogue. In FLAMSTEED'S observations 18 is marked once 5m, once 6m, and once 7m; and 19 is 7m.

20. "Oct. 19, 1784, 7m." In FLAMSTEED'S observations it stands 6m.

21 Is less than 17, contrary to the catalogue. It is also contrary to the magnitudes given in the observations. If there be any accuracy in the magnitudes of the catalogue and of the observations, we ought to look upon this star as changeable; for the latter give it once 3m, and once 6m, while the former has 5m.

27. "Sept. 6, 1784, 6m." In FLAMSTEED'S observations there is no magnitude of this star.

31 Is no larger than 50, contrary to the catalogue, "Sept. 5, 1784, 6m," and "Oct. 19, 1784, 5.6m." FLAMSTEED'S observations give it twice 5m, and 50 also 5m.

32. "Sept. 8, 1784, 6.5m." In FLAMSTEED'S observations it stands once 4m, and once 5m.

42 Is less than 48, contrary to the catalogue. But in the observations there is no magnitude to 48. "Sept. 19, 1784, 48 (μ) 4.3m."

43 Is less than 56, contrary to the catalogue. It is also

contrary to the magnitudes in the observations, where 43 is 4m, 56 5m.

47. "Sept. 19, 1784, 4.3m." FLAMSTEED's observations give no magnitude.

62 Is larger than 56 and 78, contrary to the catalogue. In the observations 56 is 5m.

63 Is less than 67, contrary to the catalogue. The observations give no magnitude of these stars.

68 Is larger than 70, contrary to the catalogue. In the observations 68 is without magnitude, and 70 is 5m and 6m.

76 Is less than 74, contrary to the catalogue. In the observations both stars are marked 6m.

79. "Sept. 8, 1784, 6.7m." The observations give no magnitude.

86 Is less than 66, contrary to the catalogue, and contrary to the observations, where the former is marked 5m, the latter 5m, and twice 6m.

88 Is less than 44, contrary to the catalogue. There are no magnitudes of these stars in the observations. It is also less than 34 Aquarii, which FLAMSTEED has observed 3m, and hardly larger than 6 Arietis, which he has also observed 3m. Therefore, if the catalogue may be trusted where this star is 2m, it must have lost some of its former lustre. But I rather suppose that this star, as well as 53 and 54, have been overvalued in the catalogue.

Catalogue and Atlas.

The letter *t*, which FLAMSTEED has annexed to 31 in his observations page 57 and 130 should be added.

Notes to Sagitta.

" Sept. 7, 1781. Order of magnitude $\gamma\ \delta\ \begin{matrix}\alpha''\\ \beta\end{matrix}$

7 Is larger than 5 and 6, contrary to the catalogue. By the order of magnitude, it appears that 14 years ago it was also larger. In FLAMSTEED's observations 5, 6 and 7 are marked 4m.

WM. HERSCHEL.

Slough, near Windsor,
Jan. 1, 1796.

X. *Experiments and Observations on the Inflection, Reflection, and Colours of Light.* *By* Henry Brougham *Jun. Esq.* *Communicated by Sir* Charles Blagden, *Knt. Sec. R. S.*

Read January 28, 1796.

It has always appeared wonderful to me, since nature seems to delight in those close analogies which enable her to preserve simplicity and even uniformity in variety, that there should be no dispositions in the parts of light, with respect to inflection and reflection, analogous or similar to their different refrangibility. In order to ascertain the existence of such properties, I began a course of experiments and observations, a short account of which forms the substance of this paper. For the sake of perspicuity I shall begin with the analytical branch of the subject, comprehending my observations under two parts: *flexion*, or the bending of the rays in their passage by bodies, and *reflection*. And I shall conclude by applying the principles there established to the explanation of phænomena, in the way of synthesis.

As in every experimental inquiry much depends on the attention paid to the minutest circumstances, in justice to myself I ought to mention, that each experiment was set down as particularly as possible immediately after it was made; that they were all repeated every favourable day for nearly a year, and before various persons; and as any thing like a preconceived

opinion, with respect to matter of theory that is in dispute, will, it is more than probable, influence us in the manner of drawing our conclusions, and even in the manner of recording the experiments that lead to these, I have endeavoured as much as possible to keep in view the saying of the Brahmin: "that he who obstinately adheres to any set of opinions, may "bring himself at last to believe that the fresh *sandal wood* is "a flame of fire."*

PART I. *Of Flexion.*

In order to fix our ideas on a subject which has never been treated of with mathematical precision, we shall suppose, for the present, that all the parts of light are equally acted upon in their passage by bodies; and deduce several of the most important propositions which occur, without mentioning the demonstrations.

Def. 1. If a ray passes within a certain distance of any body, it is bent inwards; this we shall call Inflection. 2. If it passes at a still greater distance it is turned away; this may be termed Deflection. 3. The angle of inflection is that which the inflected ray makes with the line drawn parallel to the edge of the inflecting body, and the angle of incidence is that made by the ray before inflection, at the point where it meets the parallel. And so of the angle of deflection.

Proposition I. The force by which bodies inflect and deflect the rays acts in lines perpendicular to their surfaces.

Prop. II. The sines of inflection and deflection are each of them to the sine of incidence in a given ratio; (and what this ratio is we shall afterwards shew).

* Asiatic Researches, Vol. I. p. 224.

Prop. III. The bending force is to the propelling force of light, as the sine of the difference between the angles of inflection (or deflection) and incidence, to the cosine of the angle of inflection (or deflection).

Prop. IV. The rays of light may be made to revolve round a centre in a spiral orbit.

Prop. V. If the inflecting surface be of considerable extent, and a plane, then the curve described may be found by help of the 41. Prop. Book I. *Principia;* provided only, the proportion of the force to the distance be given. Thus, if the bending force be inversely as the distance, the curve cannot be found; for in order to obtain its equation, a curvilinear area must be squared, which in this case is a conic hyberbola; the relation, however, between its ordinates and abscissæ may be obtained in fluxions, thus; $y\dot{y} + by = a^2\dot{x}^2$.

If the force (which is most probable) be inversely as the square of the distance, the curve to be squared is the cubic hyperbola; Species LXV. genus III. of NEWTON's Enumeration; and this being quadrable, the curve described by the light will be the *parabola campaniformis pura;* Species LXIX. of NEWTON.

If the force be inversely as the cube of the distance, the curve is a circular arch, and that of deflection is a conic hyperbola.* If the inflecting body be a globe or cylinder, and the force be inversely as the square of the distance from the surface, then by Prop. 71. Book I. *Principia,* the attraction to the centre is inversely as the square of the distance from that centre; and therefore, by Prop. 11. and 13. of the same book, the ray moves in an ellipse by the inflecting, and an hyperbola

* *Principia,* Lib. I. Prop. 8.

by the deflecting force, each having one focus in the centre of the body. The truth of these things mathematicians will easily determine.

Prop. VI. If a ray fall on a specular surface, it will be bent before incidence into a curve, having two points of contrary flexure, and then will be bent back the contrary way into an equal and similar curve; as in fig. 1. (Tab. VII.)

Corollary to these propositions. If a pencil of rays fall *converging* on an interposed body, the shadow will be less than the body by twice the sine of inflection.

And if a pencil fall *diverging* on the body, the shadow will be greater than the body by twice the sine of inflection; but less than it should be, if the rays had passed without bending, by twice the sine of the difference between the angles of inflection and incidence.—The sine or angle of incidence is greater than the sine or angle of inflection, when the incident rays make an acute angle with the body; but when they make an obtuse or right angle, then the sine or angle of inflection is less than that of incidence. The sine of incidence is greater than that of deflection, if the angle made by the incident ray with the body is obtuse, but less, if that angle be acute or right.—If a globe or circle be held in a beam of light the rays may be made to converge to a focus.

Hitherto it has been supposed, that the parts of which light consists have all the same disposition to be acted upon by bodies which inflect and deflect them; but we shall now see that this is by no means the case.

Obs. 1. Into my darkened chamber I let a beam of the sun's light, through a hole in a metal plate (fixed in the window-shut) of $\frac{1}{40}$th of an inch diameter; and all other light being absorbed

by black cloth hung before the window, and in the room, at the hole I placed a prism of glass, whose refracting angle was 45 degrees, and which was covered all over with black paper, except a small part on each side, which was free from impurities, and through which the light was refracted, so as to form a distinct and tolerably homogeneous spectrum on a chart at six feet from the window. In the rays, at two feet from the prism, I placed a black unpolished pin (whose diameter was every where one-tenth of an inch) parallel to the chart, and in a vertical position. Its shadow was formed in the spectrum on the chart, and had a considerable penumbra, especially in the brightest red, for it was by no means of the same thickness in all its parts; that in violet was broadest and most distinct; that in the red narrowest and most confused, and that in the intermediate colours was of an intermediate thickness and degree of distinctness. It was not bounded by straight, but by curvilinear sides, convex towards the axis to which they approached as to an asymptote, and that, nearest in the least refrangible rays, as is represented in fig. 2. where AB is the axis, IKLMNA and HGFEDA the two outlines. Nor could this be owing to any irregularity in the pin, for the same thing happened in all sorts of bodies that were used; and also if the prism was moved on its axis, so that the colours might ascend and descend on these bodies, still wherever the red fell it made the least, and the violet the greatest shadow.

Obs. 2. In the place of the pin, I fixed a screen, having in it a large hole on which was a brass plate, pierced with a small hole $\frac{1}{42}$ of an inch in diameter; then causing an assistant to move the prism slowly on its axis, I observed the round image made by the different rays passing through the hole to the chart; that

made by the red was greatest, by the violet, least, and by the intermediate rays, of an intermediate size. Also when at the back of the hole I held a sharp blade of a knife, so as to produce the fringes mentioned by GRIMALDO and NEWTON; those fringes in the red were broadest, and most moved inwards to the shadow, and most dilated when the knife was moved over the hole; and the hole itself on the chart was more dilated during the motion when illuminated by the red than when illuminated by any other of the rays, and least of all when illuminated by the violet. Now in Obs. 1. the angle of incidence of the red rays was equal to that of the violet and all the rest, and yet the angle of inflection was greatest, and least in the violet; and indeed the difference between the two was greater than appears at first from the experiment; for that part of the shadow which was formed by the violet fell at a greater distance from the point of incidence, than did that part which was formed by the red, from the divergency of the different rays upwards by the refraction, as appears in fig. 3. where DE is the window, FG the beam propagated through the hole F, refracted by the prism KIH, and painting on the chart OP *qs*; the spectrum *vr* being separated into L*r* the red rays incident on the pin CD at C, and M*v* the violet incident at D; the shadow of DC being formed in *vr*, so that *v* being farther from D than *r* is from C, therefore (by the propositions formerly laid down) the shadow in *v* should be considerably less than that in *r*, if the rays were equally inflected. Lastly, in Obs. 2. the angle of the red's incidence was nearly equal to that of the violet's, by the motion of the prism, and the consequent motion of the colours; only that, if there was any difference, it was on the side of the violet;

and yet the violet was least inflected, and the red most inflected; and so of the second inflection by the knife blade: wherefore I conclude that the rays of the sun's light differ in degree of inflexibility, and that those which are *least refrangible* are *most inflexible*.

Obs. 3. My room being darkened as before, and a conical beam propagated through the small hole in the window-shut; at this hole I placed a hollow prism, made of broken plates of mirror, and of such an angle, that when filled with distilled water, it cast a spectrum on an horizontal table, and was there received on a chart seven feet from the window. I then placed on the same table, and in the rays between the chart and the prism, at three inches from the chart, two sharp knife-blades with even edges, and fixed to a board with wax, so as to make an angle with one another; moving them nearer and nearer, till I saw the fringes appear in the red light on the chart, and then in the orange and other colours successively. I then withdrew one, and the fringes became faint and narrow, and not all within the shadow of the remaining knife, but at its edge, and even in the light of the spectrum. Lastly, when I slowly approached the other, they moved into the shadow, and became broader, and farther separated one from another, there being the like fringes in both shadows; this I repeated in all the rays, and plainly saw that at the approach of the knife, the fringes became broader, and farther removed from one another, and from the light, in the red than in the violet, or any of the other rays.

Obs. 4. In repeating the foregoing experiment, I observed a very curious phænomenon. When the angle of the knife-blades was so held in any of the rays as to make the hyper-

bolic fringes described by NEWTON,* and these being always of the colour in which they were held, moving the angle a little, so as to make the fringes out of the light that went to the top of any one division of the spectrum and also out of that which went near the bottom of the next, the fringes were made of two colours; one part was of the highest colour, and the other of the lowest, but *both* were on the ground of the highest. Thus if held on the confine of the green and blue, the upper half of each fringe was blue, the under green, but both parts in the blue division of the spectrum; and trying the same in all the rays, it was evident that the red was moved farther into the orange, and the orange into the yellow, than the blue was into the indigo, or the indigo into the violet. Now, in Obs. 3. the fringes were formed by the *inflection* of one knife, and were moved into its shadow, and separated and dilated by the *deflection* of the other; and this most in the red and least in the violet: likewise in Obs. 4. the fringes of one colour were deflected into the region of the next, and this most in the red, and least in the violet; although in both observations the violet, from the position of the chart, was farthest from the angle, and consequently had the rays been equally deflected, the violet should have been farthest moved, and most dilated by the deflection; but seeing that at equal angles of incidence in the third, and at less in the fourth observation, the red was most and the violet less deflected, it is evident that the most *inflexible* rays are also most *deflexible*.

Having thus found that the parts of light differ in *flexibility*, I wished next to learn two things; in what proportion the angle of *inflection* is to that of *deflection* at equal incidences;

* Optics, Book III. Obs. 8.

and secondly, what proportion the different *flexibilities* of the different rays bear to one another. But the nature of the coloured fringes must first be understood, so that I defer this inquiry till after I have made use of the principles now laid down, for the explanation of natural phænomena, and proceed in the mean time to

Part II. *Of Reflection.*

That bodies reflect light by a repulsive power, extending to some distance from their surfaces, has never been denied since the time of Sir Isaac Newton.* Now this power extends to a distance much greater than that of apparent contact, at which an attraction again begins, still at a distance, though less than that at which before there was a repulsion; as will appear by the following demonstration which occurs to me, and which is general with respect to the theory of Boscovich.† In fig. 4. let the body A have for P an attraction, which, at the distance of AP, is proportional to PM; then let P move towards A so as to come to the situation P′, and let the attraction here be P′M′; as it is continual during the motion of P to P′, MM′ is a curve line. Now in the case of the attraction of bodies for light, and for one another, PM is less than P′M′, and consequently MM′ does not ever return into itself, and therefore it must go, *ad infinitum*, having its arc between AB and AC, to which it approaches as asymptotes; the abscissa always representing the distance, and the ordinate the attraction at that distance: let P′ now continue its motion to P″, and M′ will move to M″, and if P″ meets A, or the

* Optics, Book II. Part III. prop. 8.

† *Nova Theoria Philosophiæ Naturalis.*

bodies come into perfect contact, P″ M″, will be infinite; so that the attraction being changed into cohesion, will be infinite, and the bodies inseparable, contrary to universal experience; so that P can never come nearer to A than a given distance. In the case of gravity, PM is inversely as the square of AP, so that the curve NMM‴ is the cubic hyperbola; but the demonstration holds, whatever be the proportion of the force to the distance. It appears then that flexion, refraction, and reflection, are performed by a force acting at a definite distance; and it is reasonable to think even *a priori*, that as this same force, in other circumstances, is exerted to a different degree on the different parts of light, in refracting, inflecting, and deflecting them, it should also be exercised with the like variations in reflecting them. Let us attend to the proof, which enables us to change conjecture into conviction.

Obs. 1. The sun shining into my darkened chamber through a small hole $\frac{1}{40}$th of an inch in diameter, I placed a pin of $\frac{1}{30}$th of an inch diameter in the cone of light (one-half inch from the hole) inclined to the rays at an angle of about 45°, and its shadow was received on a chart parallel to it, at the distance of two feet. The shadow was surrounded by the three fringes on each side, discovered by GRIMALDO; beyond these there were two streaks of white light diverging from the shadow, and mottled with bright colours, very irregularly scattered up and down; but on using another pin, whose surface was well polished, and placing it nearer the hole than before, the colours in the streaks became much brighter (and the streaks themselves narrower), being extended from one side to the other, so that, except in a very few points here and there, no

white was now to be seen; and on moving the pin, the colours moved also. But they disappeared if the pin was deprived of its polish, by being held in the flame of a candle, or if a roll of paper was used instead of the pin; also, they were much brighter in direct than in reflected light, and in the light of the sun at the focus of a lens, than in his direct unrefracted light. Placing a piece of paper round the hole in the window-shut, I observed the colours continued there; and inclining the chart to the point where they left off, I saw them continued on it, and then proceed as before to the shadow. If the pin was held horizontally, or nearly so, they were seen of a great size on the floor, the walls, and roof of the room, forming a large circle; and if the chart was laid horizontally, and the pin held between the hole and it, in a vertical position, the circle was seen on the chart, and became an oval, by inclining the pin a little to the horizon.

Obs. 2. Having produced a clear set of colours, as in the last observation, I viewed them as attentively as possible, and found that they were divided into sets, sometimes separated by a gleam of white light, sometimes by a line of shadow, and sometimes contiguous, or even running a little into one another. They were spectra, or images of the sun, for they varied with the luminous body by whose rays they were formed, and with the size of the beam in which the pin was held; and when, by placing it between my eye and the candle, a little to one side, I let the colours fall on my retina, I plainly saw that they resembled the candle, in shape and size (though a little distended), and also in motion, since if the flame was blown upon, they had the like agitation. The colours therefore which fell on the chart were images of the

sun; they had parallel sides pretty distinctly defined, but the ends were confused and semicircular, like those of the prismatic spectrum. Like it too, they were oblong, and in some the length exceeded the breadth six, even eight times; the breadth was, as I found by measurement, exactly equal to that of the sun's image received on a chart, as far from the pin as the image was, and the length was always to the breadth at all distances, in the same ratio, but not in all positions of the pin; for if it was moved on its axis, the images moved towards the shadow on one side, and from it on the other, becoming longer and longer (the breadth remaining the same) the nearer they came to the shadow on the one side, and shorter in the same proportion, the farther they went from it on the other.

Obs. 3. Having picked out an image that appeared very bright and well defined, I let it through a hole with moveable sides, in the upper part of a sort of desk, which moved to any opening by hinges, and had a chart for its under side, on which the image fell, and I shut the hole so close as to prevent any of the others from coming through; I then had a full opportunity of examining it, in all respects, and I counted in it distinctly the seven prismatic colours; the red was farthest from the shadow of the pin, and from the pin itself; then the orange; then the yellow, green, blue, and indigo, and the violet nearest of all; in short, it was exactly similar to a prismatic spectrum, much diminished in length and breadth, and turned horizontally on the wall opposite to the prism, with the red farthest away. In fig. 5. *se* is the pin, reflecting the rays CP and CO, which pass through PO, the hole in the desk ED, to the chart or bottom of the desk RTSD, and form there the

spectrum IK divided into its colours, I being violet, and K red. On moving the hole in the desk, and letting through other images, the colours were not in all arranged the same way; but I moved the pin on its axis, and observed those where the order was inverted to move, not only with respect to the pin, but also with respect to the contiguous images; and I was surprised to see them assume the order of colours first mentioned, namely, the red outermost, and the violet innermost. In like manner the images, which before the motion were regular, on moving into the places left by the others had always the order of their colours inverted, so that the thing must be owing to some irregularities in the pin's surface; for those which were made by a small glass tube filled with quicksilver, and freed from scratches by a blow-pipe, preserved during the motion the proper order of colours. Another irregularity in the arrangement was also observable even in the glass tube; for two contiguous images, by mixing one with another for two or three successions, appeared each to have outermost a dull colour, between red and violet, and innermost a green; but here, unless the succession continued through all the images, the outermost of all was red, and the innermost image had universally violet in the inside.

Obs. 4. I placed at a hole in the window-shut a prism, to refract the rays, and received the spectrum at the distance of six feet from the window, on a chart; then, at the distance of two feet, I placed a screen with a hole in the middle of it, through which I let pass successively the different rays. At the distance of one inch from the hole, between it and the chart, I placed the reflecting cylindrical body; the images were found on the chart and walls of the room round to the sides

of the hole on the screen, and were always wholly of the colour in which they were formed, except in the confines of the green, where a small quantity of white light fell, and made them of all the seven colours; but this was almost wholly prevented by using a prism with a greater refracting angle, and holding the pin and screen farther from it. I then removed the screen, and left the reflector in its place, so as it might reach through the rays; and thus there were formed images, having in them, from top to bottom, the seven colours, one after another, the lowest division being red, the highest violet. They were inclined considerably towards their tops, and were much broader at the bottom or red parts than at the tops or violet parts. And lastly, the reflector being moved so that the images might be disturbed (as in the former experiment made in the white light), the red was most, the violet least dilated. In case these effects might be owing to any peculiarities in the shape or position of the reflector, I placed at three feet from the prism a lens of four inches breadth, to collect the rays to a focus, six feet beyond which I held a chart, and there received the spectrum inverted, the red being uppermost, and the violet undermost; holding the reflector at two feet from the focus, and four from the chart, the images were formed just as before, only inverted, inclining towards the violet, of greater breadth towards the red, and more distended towards the same quarter when the reflector was moved.

Obs. 5. Things remaining as in the last part of the last experiment, at the focus of the lens I placed a second prism, which refracted the rays into a white beam,* and this I

* Optics, Book II. Part II. Prop. 2.

received on a screen with a hole in the middle, through which a small part of it passed, and falling on the reflector placed behind, was formed by it into images, after the manner of the first experiment, each having in regular order the seven prismatic colours. One of the brightest and most distinct I let pass through a hole in a second screen, and it fell on the chart. I then caused an assistant to intercept the red rays between the first prism and the lens, and immediately the red part of the image vanished; and when the violet was intercepted, the violet of the image vanished; and if the green was intercepted, the green was wanting in the image. In short, whatever colours were stopped, the same were missing in the image. In fig. 6, the rays passing through the hole C of the window AB, are refracted by the prism PMN, and separated into DV, DG, and DR, violet, green, and red; which being collected into a focus F by the lens L, are there again refracted by a prism P′M′N′, and formed into a white beam *abmn*, part of which is intercepted by the screen SS′, and part passes through the hole *h*, as *h*H to H on the chart XYZW, and part is reflected by the body *oq* into a set of images which are received on a screen TU, and one of them, *rgv*, let pass to WXYZ; but when an obstacle E stops DR, *r* the red vanishes; and if DG be stopped, *g* the green vanishes; and if DV be stopped, *v* disappears. Lastly, if DR and DG be stopped, *g* and *r* vanish.

Obs. 6. Having produced a set of bright images, I let one pass through the desk described in the third experiment, and received it on a small lens $\frac{1}{2}$ inch broad, to collect them into a focus, which I received on the chart, by moving it a little on its hinge; and by all the observations I could make, and all the

tests I could think of, it was white inclining to yellow, and of the same nature and constitution with the sun's direct light; but if any ray was stopped before coming to the lens, the focus was a mixture of the remaining rays; and the chart being moved a little farther round, the image was formed on it, the colours being in an inverted order. At the focus I held a reflector, and there were formed images of all the seven colours, as in the sun's direct light (Exp. 1.); if the light was sufficiently strong, and the desk near the window-shut hole, one of these could even be collected by a second lens into a white focus. This experiment is rendered more uniform by substituting for the lens a concave metallic mirror, and placing at the focus another mirror to reduce the rays into a beam, which may be made of any composition we please, by stopping one or more of the colours at the hole in the desk. I observed in the course of these experiments a phænomenon worth mentioning; if a comb (as in NEWTON'S experiment*) be very swiftly moved before one of the images, or more, a sensation of white is produced; but this is still more evident, if the pin be swiftly moved round its axis, for then the images move also, and running into one another, cause a sensation of perfect whiteness.

Obs. 7. I let an image through the hole in the desk, and viewed it through a glass prism, holding its axis parallel to the sides of the image, and its refracting angle upwards; I found that, if the image was bright and free from white light, the colours were not changed by the refraction; but, if it was mixed and diluted with white, the prism, decompounding the white, caused the image to appear violet at one side, and red

* Optics, Book I. Part II. Prop. 5.

at the other; yet still this only confused the colours of the image, without changing them. Farther, if the prism was moved on its axis, the violet was lifted higher than the red or any of the other colours. Nor was the constitution of the colours at all changed by reflection from a pin or mirror, except in so far as they were mixed by a concave one, as mentioned in the last experiment. If a pin was held behind the hole to reflect the colours, it formed other images of the colour in which it was held, and, as far as I could judge, threw the red to the greatest distance, and breadth, and inclination. Nor were the colours of the image changed by reflection from natural bodies, for these were all of the colours in which they were held, but brightest in that which they were disposed to reflect most copiously. Likewise the rings of colours made by thin plates were broadest in the red, and narrowest in the violet; and the like happened to the fringes that surround the shadows of bodies. Lastly, the shadows of bodies were themselves broadest in the violet, and narrowest in the red.

Obs. 8. I filled with water a glass tube, whose diameter was $\frac{1}{4}$th of an inch, and consequently the radius of curvature $\frac{1}{8}$th, and whose sides were $\frac{1}{30}$th of an inch thick; then standing at four feet from a candle, I held the tube $\frac{1}{4}$th of an inch from my eye, so that the light of the candle might be refracted through it, and moved my eyelids close enough to prevent the extraneous scattered light from entering along with that which was regularly refracted. I saw several images of the candle all highly coloured, and the colours were in order, from the candle outwards, red, orange, and so on to violet; I then filled the tube with clear diluted sulphuric acid, and dropped a small piece of chalk to the bottom, when immediately an

effervescence took place, by the escape of fixed air, which rose in bubbles through the tube; and looking at the candle through one of these, I saw the images formed with the colours still in the same order, but a little larger than before.

We are now to see to what conclusions these experiments lead us.—The first experiment shows, that all sorts of light, whether direct, or reflected, or refracted, produces colours by reflection from a curve surface. From the second we learn, that these colours are distinct images or spectra of the luminous body, much dilated in length, but not at all in breadth; and that the angle of incidence being changed, the dilatation of the images is also changed: and from the third experiment it appears, that each full image is composed of seven colours; red, orange, yellow, green, blue, indigo, and violet; and that the proper order is red outermost, and violet innermost, the rest being in their order. The fourth experiment shows, that these images are produced, not by any accidental or new modification impressed on the rays, but by the white light being decomposed by reflection; that the mean rays, or those at the confine of the green and blue, are reflected at an angle equal to that of incidence, and the red at a less, the violet at a greater angle. Experiments 5th and 6th prove, beyond a doubt, the decomposition and separation of the rays by reflection; for in both we see that the colours in the images are those, and those only, which were mixed in the ray by reflection or refraction, before and at incidence, whilst the 6th is (in addition) a proof that all the rays of any one image, if mixed together, compound a beam exactly similar to the beam that was at first decompounded. The 7th experiment shows, that the colours into which the rays are separated by reflection are homogeneous

and unchangeable; that they differ in flexibility and refrangibility; that they bear the same part in forming images by reflection, and fringes by flexion, and colours from thin plates, which the rays separated by the prism do: and in the 8th experiment we see, that when the rays are placed in the same situation with respect to refraction, whether out of a rarer into a denser or a denser into a rarer medium, in which they before were with respect to reflection, the position of the colours produced is diametrically opposite in the two cases. Seeing then that in all sorts of light, direct, refracted, reflected, simple, and homogeneous, or heterogeneous and compounded, and in whatever way the separation and mixture may have been made, some of the rays at equal or the same incidences are constantly reflected nearer the perpendicular than the mean rays, and others not so near; and seeing that by such reflection the compound ray, of whatever kind, is separated into parts so simple that they can never more be changed; and considering the different places to which these parts are reflected; it is evident, that the sun's light consists of parts different in reflexibility, and that those which are least refrangible are most reflexible. By reflexibility, I here mean a disposition to be reflected near to the perpendicular in any degree.

Although I have given what I take to be sufficient proof of this property of light, yet I am aware that something more is requisite. It will be asked, why does neither a plain, a common convex, nor a common concave mirror separate the rays by reflection? This is what has always hindered us from even suspecting such a thing as different reflexibility. I shall, however, take an opportunity of removing this obstacle, in the second part of the plan, when I come to explain the reason of

the colours made by the reflecting body, and the manner of their formation. At present I shall only caution those who may wish to repeat the above experiments, that the hole in the window-shut must be small, the room quite dark, the pin well polished, and the desk, chart, &c. placed at a distance from the pin not greater than three feet, otherwise the images will be dilute and dim; nor, on the other hand, less than six inches, otherwise they will be too short, and the colours not far enough separated one from another.

My next object of inquiry was the different degrees of reflexibility belonging to each ray. It appears, not only from mathematical considerations sufficiently obvious, but also from the experiments I have related, that though the different rays have at the same or equal incidences different angles of reflection, yet each ray is constant to itself in degree of reflexibility, and that its sine of reflection bears always the same ratio to its sine of incidence. The question then is, what are the sines of reflection of the different rays, the sine of incidence being the same to all?

Obs. 9. In summer, at noon, when the sun's light was exceedingly strong, and there was not the vestige of a cloud in the sky, I produced an uncommonly fine set of images, by fixing at an inch from the small hole $\frac{1}{50}$th of an inch diameter, a pin $\frac{1}{25}$th of an inch diameter. One of the brightest of these I let pass through the desk to the chart below at $2\frac{1}{2}$ feet from the pin, and the image was 3 inches from the shadow in a straight line. I delineated it carefully, by drawing two parallel lines for the sides, and marking the semicircular ends. Then with the point of a small needle I marked the confines of the contiguous colours on one of the parallel sides, and

afterwards drew across the image parallel lines; this operation I repeated with the same and different images, at many distances from the pin, and on different days, with various sorts of pins, and sizes of holes, &c. &c. and all these repetitions were made before I once examined the result of any one measurement, that I might be unprejudiced in trying the thing over again. I then compared the sketches of divided images, which I thus obtained, and found sufficient reason to conclude, that the differences between the sines of reflection in the different rays were in the harmonical order. For the divisions were nearly as $\frac{1}{9}$; $\frac{1}{18}$; $\frac{1}{12}$; $\frac{1}{12}$; $\frac{1}{15}$; $\frac{3}{80}$, $\frac{1}{16}$; which, when compounded with the scale, give 1, $\frac{15}{16}$, $\frac{9}{10}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{11}{18}$, $\frac{1}{2}$; and these are exactly the change of the notes in an octave, obtained by taking the sums of the octave, and a second major, a third major, a fourth, a fifth, a sixth major, a seventh major, and an eighth, instead of the difference between a double octave, and a second major, a third major, and so on. Thus the spectrum by reflection is divided exactly as the spectrum by refraction, only that the former is inverted, and the different rays have reflexibilities that are inversely as their refrangibilities.

Having settled this (I flatter myself) curious and important point, I proceeded next to inquire into the absolute reflexibility of the extreme colours; for if this be known, the angle of incidence being given, the angle of reflection of all the different rays may be found. For obtaining a solution of this problem I made the following experiment.

Obs. 10. The sun shining strongly through the small hole in the window-shut, and the rays diverging into a cone, whose base fell on an horizontal chart $2\frac{1}{2}$ feet from the hole, between the hole and chart I placed a screen, which had a plate

and small hole in it; the rays passing through this, fell on a small pin, so placed that the images formed might be at right angles to the shadow; one of these I measured, together with its distance from the shadow, the distance of the shadow from the hole, the breadth of the shadow, and the diameter of the pin; these measures were as follows. In fig. 7. C is the centre, and B*en* the circumference of the pin, GM the chart, and GD a line in it, being the axis of all the images, at right angles to CD, the distance of C from D the centre of the shadow, and also to the shadow itself; GE is the parallel side of the image, G being red, E violet, and F the confine of the green and blue; C*e* is a radius parallel to ED, and CA another drawn through B, the point where OB is incident, at the angle OBA, to which (by what was before shown) ABF is equal. By measurement GE is $\frac{1}{4}$th of an inch, CB $\frac{1}{80}$th, CD $4\frac{1}{2}$; now the shadow being lessened by a penumbra, this added to half the shadow, and their sum to the distance between the penumbra and the violet, gave ED $\frac{117}{400}$th of an inch. From whence it is easy to calculate, that the angle of incidence being 77° 20′, the angle of the red's reflection ABG is 75° 50′, and that of the violet's 78° 51′. Now the natural sines of 77° 20′, 75° 50′, and 78° 51′, are as 9756, 9695, and 9811; or as 250, 248, and 251; which are very nearly as $77\frac{1}{2}$, 77, and 78; and making an allowance for the omissions made in the reductions, the errors in the operations and measurements, they may be accounted as accurately in the above proportion. Now these extremes, 77 and 78, are the very proportions of the red's refrangibility to the violet's.* So that the reflexibility of the red is to that of the violet as the re-

* Optics, Book I. Part I. Prop. 7.

frangibilities inversely. But it is obvious that the sine of incidence is not the same in the two cases; for in the one it is equal to that of the mean ray's reflection, while in the other none of the rays are refracted at an angle equal to that of incidence, otherwise they would not be refracted at all. This, however, being a consequence of the essential distinction in the circumstances, does not impair the beautiful analogy which we have seen is preserved in the two operations, and which proves them to be different exertions of the same power. Now we may find, from the data obtained, the sines of all the rays in the spectrum, by adding to 77 the lengths of the spaces into which it is divided, and which are without any sensible error as the differences of those sines. The sines of the *red* will be from 77 to $77\frac{1}{8}$; the *orange* from $77\frac{1}{8}$ to $77\frac{1}{5}$; the *yellow* from $77\frac{1}{5}$ to $77\frac{1}{3}$; the *green* from $77\frac{1}{3}$ to $77\frac{1}{2}$; the *blue* from $77\frac{1}{2}$ to $77\frac{2}{3}$; the *indigo* from $77\frac{2}{3}$ to $77\frac{7}{9}$; the *violet* from $77\frac{7}{9}$ to 78. So that the sine of incidence being given, that of the reflection of all the different rays may be found; and the angle of incidence being 50° 48′, the angles of reflection are as follows: of the extreme red 50° 21′; of the orange 50° 27′; of the yellow 50° 32′; of the green 50° 39; of the blue 50° 48′; of the indigo 50° 57′; of the violet 51° 3′; and of the extreme violet 51° 15′.

I shall conclude this part of the subject with a few remarks on the physical cause of reflexibility. As light is reflected by a power extending to some distance from the reflecting surface, the different reflexibility of its parts arises from a constitutional disposition of these to be acted upon differently by the power. And as these parts are of different sizes, those which are largest will be acted upon most strongly. I shall not hesitate to go a

step farther. In fig. 8. let EC be the reflecting surface, DH the perpendicular, and AB a ray incident at B, and produced to F, and reflected into GB; draw GH parallel to FB, and GF to HB. Then HB : (HG :) BF : : sin. HGB : sin. HBG, or : : sin. GBF : sin. HBG. But GBF is the supplement of GBA, the sum of the angles of reflection and incidence; wherefore HB : BF : : the sine of the sum of the angles of reflection and incidence, to the sine of the angle of reflection; so that if I be the angle of incidence, R that of reflection, V the velocity of light, and F the reflecting force; $F = \frac{V \times \text{sin. } (R+I)}{\text{sin. } R.}$. By accommodating this formula to the different cases, we obtain F in all the rays; and the ratio of F in one set to F in another being required, we have (by striking out V, which is constant) $F : F' : : \frac{\text{sin. } (R+I)}{\text{sin. } R} : \frac{\text{sin. } (R'+I')}{\text{sin. } R'}$. Suppose we would know F and F′ in the red and violet respectively; $I = 50^\circ\ 48'$ $- R = 50^\circ\ 21'$, and $R' = 51^\circ\ 15'$; then $F : F' : : \frac{\text{sin. } 101^\circ\ 9'}{\text{sin. } 50^\circ\ 21'} : \frac{\text{sin. } 102^\circ\ 3'}{\text{sin. } 51^\circ\ 15'}$. Performing the division in each by logarithms, and finding the natural sines corresponding to the quotients; F : F′ : : 1275 : 1253. But the force exerted on the red is to that exerted on the violet, as the size of the red to the size of the violet (by hypothesis); therefore, the red particles are to the violet as 1275 to 1253. This may be extended to all the other colours, by similar calculations; their sizes lying between 1275 and 1253, which are the extreme red and extreme violet; thus the red will be from 1275 to $1272\frac{1}{2}$; the orange from $1272\frac{1}{2}$ to 1270; the yellow from 1270 to 1267; the green from 1267 to 1264; the blue from 1264 to 1260; the indigo from 1260 to 1258; and the violet, from 1258 to 1253.

All this follows mathematically, on the supposition that the parts of light are acted upon in proportion to their sizes; and to say the truth, I see no other proportion in which we can reasonably suppose them to be influenced; for such an action is not only conformable to the universal laws of attraction and repulsion, but also to the following arguments. If the action be not in the simple ratio, it must either be in a lower or in a higher; let it be in a lower, as that of the square root, then the size of the red would be to the size of the violet as the squares of the forces; that is, as 1625625 to 1572009: a difference evidently too great; and, *a fortiori*, of the cube or any other root. On the other hand, if the action were in a higher ratio, as that of the square, then the particles would be as the square roots of the forces, or nearly as 35.70 to 35.39, a difference evidently too small; for if the size of the red particles were only $\frac{3}{10}$ths greater than that of the violet, and the velocity of both were equal, the momentum, and consequently the intensity of the red, could not so much exceed that of the violet as we find it does, and as seems to me to be proved by the experiment of BUFFON (on accidental colours), who found, that after looking at a white object, when he shut his eyes, it first became violet, then blue, or a mixture of blue and the other colours, and last of all red; so in the impression of the white, compounded of the impressions of all the other rays mixed together, the violet was first obliterated or weakest, and the red last or strongest. To this reasoning on the intensity of the particles as owing to their size, I see only two objections that can be made. The one is, that the intensity is increased when the rays are thrown into a focus; but we must recollect that the rays in this case are mixed, and their

particles so blended as to be increased in size; for the number of separate rays thrown into one place will not increase their intensity sensibly. The other objection is, that passage in NEWTON, where he says "that the orange and yellow are the "most luminous of all the colours, affecting the senses most "strongly."* Now, besides that this is an assertion opposed by the positive experiment just now quoted, I think an answer may be thus made to it; the whole light, from which the spectrum is never free, which inclines to yellow, and which is composed also of red, abounds in the yellow and orange of the spectrum; so that both of these colours derive their superior lustre rather than intensity from this circumstance; or if they have any degree of the latter more than the red, it is in fact owing to their mixture with the red and the other rays, which are all in the white.

Having endeavoured to unfold the property of flexibility, as varied in inflection, deflection, and reflection; and also the physical cause of this property; and having indulged in a speculation depending on this cause, I flatter myself neither altogether useless nor unimportant, I hasten now to the natural phænomena, the explanation of which depends on the property, whose existence and nature we have just now been investigating; and that we may treat this part of the subject with conciseness and order, we shall rank the phænomena under a division similar to that under which we laid down the principles, beginning with those appearances which are explicable on the principles of flexion.

1. It is observable, that when a body is exposed in the sun's light, so as to cast a shadow, and another body is ap-

* Optics, Book I. Part I. Prop. 7.

proached to it, either between the sun and it, or the shadow and it, or on the same line with it, the shadow of the one body comes out a considerable way, and meets that of the other. Now it is evident, that when the bodies are held at a sufficient distance from one another, a penumbra is formed round the shadow of each, making it less than it should be were there no inflection; but when the bodies are brought so close to one another that the edge of the one is within the sphere of the other's inflection, the light being already bent by this last, the former can have none to bend, and consequently no penumbra in the part of the shadow corresponding to that part of the body which is within the other's sphere of inflection; and the rest of the shadow having a penumbra, this part that has none will be larger than it, and increase as the bodies approach, till at last it meets the other shadow; the like appearance happening when the shadows are thrown on the eye. Mr. MELVILL has endeavoured to show that it belongs simply to a case of vision;* however, we have now seen that it has no reference to the structure or position of the eye, but only to the common nature of all shadows.

Obs. 11. If we shut out all the light coming into a room from external objects, except what may pass through a small hole of $\frac{1}{2}$ or $\frac{1}{4}$th of an inch in diameter, the images of the external objects, as clouds, houses, trees, will be painted on the opposite wall, by the rays of light crossing at the hole; but if a piece of rough glass, or of very fine paper, be held so as to cover it all over, the light does not pass through; then if the paper be wetted with oil, or the glass with water, so as to give either a small degree of transparency, the first rays that come

* Edinburgh Literary Essays, Vol. II.

through are those from red and orange objects, and last from blue and violet. Now it is evident that transparency in general, and this particular fact, are explicable by what was before laid down. It was found by NEWTON, that a body transmits the light incident on it more or less, according to the continuity of its particles, and that a strong reflection takes place on the confines of a vacuum.* How does this happen? The initial velocity of light is sufficient to carry it through the first surface or set of particles, but it is so much diminished, that it is reflected by the repulsive power of the backside of these particles, unless there be others behind at a certain distance, namely, that at which inflection or attraction acts, that is, apparent contact; this attraction renews the impetus of light, and transmits it to another set, and so on. Now this action being strongest on the largest and red particles, and weakest on the blue and violet, if the continuity be diminished, the former will be transmitted, and not the latter; which is conformable to the experiment just now mentioned.

3. The doctrine of flexibility furnishes an easy and satisfactory explanation of the different colours which are assumed by flame. Whether we suppose the light to come from the burning body, or the oxygenous gaz, the largest or red particles have the strongest attraction for bodies, the violet the weakest; when therefore the gaz and the body combine, the precipitation of light must be in the reverse order of the affinity between the particles of light and those of the bodies. If then the combination take place slowly, the violet and blue particles will be first emitted, and last of all the red; and this is consistent with fact; for any inflammable body whatever,

* Optics, Book II. Part III. Prop. 3.

on being lighted, burns at first with a blue or violet flame, and afterwards has its flame of two or three distinct colours, blue, white, red, &c. as is seen remarkably in the case of a candle. Nay, I have observed in the flame of a blow-pipe all the seven primary colours at once. When, indeed, a body is burnt in pure oxygenous gaz, the combination is so rapid, that white light alone is precipitated undecomposed; but in common air, where the azotic gaz impedes the combustion, the above phænomena are obvious.

4. A curious phænomenon has often surprised philosophers, namely, blue shadows. These I have observed at all times, when the paper on which I received them was illuminated by the sky, and any other light; and the reason of them I take to be this, that the shadow made by one light is illuminated by the blue rays from the sky; for I have often observed purple, and even reddish ones, when the sky or clouds happened to be of those colours; and this account of the matter is confirmed by an experiment. Having received the coloured spectrum made by a prism with a large refracting angle, on a sheet of rough white paper, and held above it another sheet, I stopped all the rays that illuminated the first except the blue, and violet, and red; and if I held a body between the blue and the second paper, its shadow was red; and if I held a body between the red and the paper, its shadow was blue; and so of other colours. This I take to amount to a demonstration of the thing.*

* Since writing the above, I find the same explanation of the matter given by Mr. Melvill, and some of the French academicians, particularly Messieurs Buffon and Beguelin; also Count Rumford; but I have thought fit to keep it in, on account of the experiment that occurred to me in illustration of it.

5. Passing over other phænomena of less note, I come now to one that has divided opticians more than any other; I mean the coloured fringes that surround the shadows of bodies. I made several observations on these, which enable me to conclude that each fringe is an image of the luminous body; for holding between my eye and a candle two knife blades, as I approached the one to the other, the edge of the candle seemed multiplied, and soon became coloured, coming wholly away from the candle, and as the knives approached still nearer, became distinct dilated images, highly tinctured with the prismatic colours; and just before the knives met, the candle, whose edges had been all along coloured with red and yellow, became much distended, till at last it was divided in the middle, one half seeming to be drawn away by each knife, and then it wholly disappeared. I have observed three kinds of these images; two without and one within the shadow; the first had its colours in the order from the shadow, red outermost, and violet innermost; the second and third had the colours in the contrary order, but the second was so very faint that I could never perceive it unless when let fall on my eye. All this is easily explained by the different flexibility of the rays. In fig. 9. let AD be a body, by which the rays SDT and S′D′T′ pass; and let SD be within AD's sphere of inflection, and S′D′ within its sphere of deflection; then SD will be bent into DG, but because of the different inflexibility of its parts, the red will be bent into DR, and the violet into DV, and the intermediate rays will fall between R and V, the whole forming an image RGV, separated into the seven primary colours; and in like manner, by the different deflexibility of the parts whereof S′D′ consists, an image without the

shadow, as V′G′R′ will be formed, similar to VGR, R′ being red and V′ violet, all which is both theory and experience; and the same explanation may be extended to the other cases. Now, in all these, the bending power stretching to a very small definite distance, and being of different degrees of strength at different distances from the body, several pencils or small beams, passing through different parts of the spheres, will be acted upon by the power in its different states of strength; and each beam will be disposed into an image in the way before described; of these images I have sometimes observed four, and even, by using great care, the faint lineaments of a fifth. In forming them, the power acts strongest at the smallest distances, and of consequence bends the mean flexible rays, that pass near, farther inwards or outwards than those that pass farther off; so that the extreme rays will in the former case be more separated from the mean than in the latter; and the nearer image will always be the largest and most highly coloured, which is consistent with fact. This explains fully the celebrated experiment of Sir ISAAC NEWTON with the knives, and the explanation is confirmed by the experiments which I related above on flexibility, where the bending force acted most strongly on those images formed out of red light, and least strongly on those formed out of violet and blue light. A number of other phænomena are explicable on the same principles, being only particular cases as it were of the coloured fringes or images; I shall here mention a few of the most remarkable.

6. When making some of the experiments which I have related in the course of this paper, I observed that when the sun was surrounded, but not covered, by clear white clouds,

the white image on the chart (the hole being $1\frac{1}{2}$ inch in diameter) was surrounded by two rainbows, pretty broad and bright; in the colours were red on the outside, and violet next the white of the image. These bows must not be confounded with one which sometimes appears wholly of a dull red and yellow, when the sun or moon shines through a cloud, and which is owing to the direct transmission of the red rays and reflection of the others; for not only are the colours different in species, in brightness, and in number, in the phænomena under discussion, but likewise they are formed by the hole in the window, as I knew by altering its shape into an oblong; and the colours now were not disposed in circles, but in broad lines of the same breadth as the bows had been, running along the shadow of the hole's sides, and in the same position of colours as before. It is evident that their cause is the inflection of the light which comes from the clouds by the sides of the hole (for if the sky have no clouds the colours do not appear), which separate the white light into the parts of which it is composed.

7. It is observable, that when we look at any luminous body, at a distance greater than one or two feet, its flame appears surrounded by two bows of faint colours, the innermost of them terminating in a white which continues to the flame; and the colours are red outermost, and green and blue innermost: the appearance is most remarkable if we look at a small hole in the window-shut, the room being otherwise dark; and if the eye be pressed upon, and then opened, the colours are more lively than before, as DES CARTES observed;* from which both he and NEWTON concluded, that the appearance

* *De Meteoribus.*

was owing entirely to wrinkles formed on the surface of the eye by the pressure.* But this could neither form the bows with the regularity in which they always appear, nor could the colours be in the order above mentioned from the different refrangibility of the rays; it will also be obvious to any one who tries the thing, that the pressure only increases the brightness and breadth of the bows, but does not form them. The true solution of the difficulty seems to be this: the rays which enter the pupil, are inflected in their passage through the fibres, which extend over the cornea, and which are very minute, but opaque; by these they are decompounded into fringes, having the red outermost, and the violet innermost; and the fringes formed by each fibre being joined together, form the bow. How then does the pressure enlarge and vivify them? The fibres are naturally extended over the surface of a spherical segment; when this surface is compressed into a plane circle, they are condensed into a much less space, and consequently brought nearer to one another, the rays are therefore more inflected and separated than before. If this explanation be true, it will follow, that the like bows may be produced by small hairs, like fibres, placed near one another; and this I found perfectly consistent with fact; the bows are in this case brighter than in the other; and the small hairs on a hat, or the hand, made them brighter than any other I have tried: a circumstance which I observed in both cases, seems to show clearly the identity of the causes; the white space, which reached from the interior bow to the flame, was speckled or mottled, in a manner which cannot be easily described, but which any one will perceive upon trying the experiment.

* *Lect. Opticæ, Sect. III. ad finem.*

8. The last of these phænomena, which I shall mention, is the celebrated one observed by Sir ISAAC NEWTON, namely, the rings of colours with which the focus of a concave glass mirror is surrounded. Sir ISAAC made several most ingenious and accurate experiments to investigate their nature;* and finding their breadth to be in the inverse subduplicate ratio of the mirror's thickness, he concluded that they were of the same nature and original with those of thin plates, described by him.† The Duc de CHAULNES pursued these experiments with considerable success; he found that the rings were brighter the nearer to the perpendicular the rays were incident; and that if, instead of a concave glass mirror, a metal one was used, with a small piece of fine cambric, or reticulated silver wire stretched before it, the colours were no longer disposed in rings, but in streaks, of the same shape with the intervals between the threads; hence he concludes that they are owing to inflection; that in passing through the first surface, they are inflected and condensed by the second.‡ I am not, I own, quite satisfied with this account of the matter: that they are produced by inflection, the Duke's experiments put beyond doubt; but that they should be formed in passing through the first surface, and reflected by the second, is quite inconsistent with the ratio observed by their breadth, this being greater in the thinnest glass, and also with the order of the colours. Besides, all the coloured images which fall on the backside of the mirror, will be (by what we before found when speaking of flexibility §) reflected into a white focus; so

* Optics, Book II. Part IV.

† Book II. Parts I, and II.

‡ *Mém. de l'Académie, pour l'année* 1755.

§ Part II. Obs. 6 of this paper.

that, upon the whole, there appears every reason to believe that the rings are formed by the first surface, out of the light which, after reflection from the second surface, is scattered, and passes on to the chart. It will follow, 1. that a plane mirror makes them not, for the regularly reflected light, not being thrown to a focus, mixes with the decompounded scattered light, and dilutes it. 2. That the nearer to the perpendicular the rays are incident, the more light will be reflected to the focus, and consequently the less will dilute and weaken the rings. 3. That the thinner the mirror is, or the nearer the two surfaces are, the broader will the rings be. 4. That the rings farther from the focus will be broader. And lastly, that when homogeneous light is reflected, the fringes or images will be larger, and farther from one another, in red than in any other primary colour. All which is perfectly consistent with the experiments of NEWTON and CHAULNES. There is only one difficulty that may be started to this explanation: how happens it that the colours (made by the mirror) are always circular? We answer, it is owing to the manner of polishing the concave mirror, which is laid between a convex and concave plate, and then turned round (with putty or melted pitch) in the very direction in which the rings are. If it should be asked, why does the thickness of the mirror influence the breadth of the rings exactly in the inverse subduplicate ratio? We answer, that to a certain distance from the point of incidence (and the rays are never scattered far from it) this is demonstrable, to hold as a property of mathematical lines in general.

Having found that the fringes by flexion are images of the

luminous body,* I thought that, from this consideration, a method of determining the different degrees of flexibility of the different rays might be deduced, similar to that which I had formerly used for determining their degrees of reflexibility.† I therefore made the following experiment.

Obs. 12. Having let into my darkened chamber a strong beam of the sun's light, through a hole $\frac{1}{40}$th of an inch in diameter, I held a hair at four feet from the hole, and receiving the shadow at two feet from the hair, I drew a line across the middle of the coloured images, and pointed off in each the divisions of the colours, as nearly as I could observe; and repeating the observation several times and at different distances, I found, by the same way I had formerly done in my experiment on reflexibility, that the axis, or line, drawn through the middle of each, was divided inversely, according to the intervals of the cords which sound the notes in an octave, *ut*, *re*, *mi*, *sol*, *la*, *fa*, *si*, *ut*. But as the measures in these experiments were very minute, and the operations of consequence liable to inaccuracy, I thought proper to try the thing by another test.

Obs. 13. The sun shining into the room as before, I placed at the hole an hollow prism made of fine plate-glass, and filled with pure water, its refracting angle being 55°; the spectrum was thrown on an horizontal chart eight feet from the window, and at four feet from the prism there was placed, in the rays, a rough black pin $\frac{1}{20}$th of an inch in diameter. The shadow in the spectrum was bounded by hyperbolic sides, as before described; and drawing a line, which might be the axis

* Page 256. † Page 247.

of the shadow, and pass precisely through its middle, I marked on one side 6 or 8 points of the shadow's outline, in each set of rays; and this being often repeated, at different distances and in different shadows, the position of the axis remaining the same, the curves formed by joining the points were all parallel; which shows that each sine of inflection taken apart has a given ratio to the sine of incidence. I afterwards divided the axis according to the musical intervals, and thus found where each colour of the spectrum had terminated, in what colour each part of the shadows had been, and by what rays formed. Then I joined the parts that I had marked, and obtained a curve, which I took to be, either nearly or accurately, an hyperbola of the 4th order. I next measured the ordinates (the axis of the spectrum and shadow being the axis of the curve) at the confines of each colour; first, the ordinate at the extremity of the rectilinear red, then that at the confine of the red and orange, and so on to that at the extreme rectilinear violet; to each of these ordinates I added the greatest one, or that in the violet, which (in fig. 10.) is VV′; that is, I produced *v*V to V′, so that *v*V′ is equal to *v*V; and through V′ I drew V′R′ parallel to the axis VR, and produced *g*G to G′, and *r*R to R′; then from V′ I set off V′*g*′ equal to G′*g*, and V′*r*′ equal to R′*r*, and the other ordinates in like manner; and I found, according to the method before described,* that VV′ was divided inversely, after the manner of the musical intervals. It is therefore evident that the inflexibilities of the rays are directly as their deflexibilities, and reflexibilities, but inversely as their refrangibilities. The same may be proved, by measuring and dividing the images made in the inside

* Page 247.

of the shadows; these I have found to be, at equal incidences and distances, equal to the images on the outside, both in breadth, in distance from the edge of the shadow, and in the relation which their divisions bear to one another; wherefore, whatever be the ratio of the angle of inflection to that of incidence, the same is the ratio of the angle of deflection to that of incidence; so that the angle of deflection is equal to the angle of inflection. If farther proof of this proposition be desired, the following experiment and observations, which from the importance of the thing I do not scruple to add, may be sufficient.

Obs. 14. When two knife blades were placed by one another in a beam of light which entered the dark room, so that the one might form and the other distend the images, I made in one of the blades (with a file) a small dent, which, on the chart, cast an elliptic or semicircular outline; then I observed that the images of both blades were disturbed by it, and wound round the edges of the semicircle; and they were all affected in precisely the same manner and degree. So then the first knife deflected the images formed by the second, in precisely the same degree that it inflected those images which itself formed, and so of the other knife; otherwise the effect of the dent would have been different upon the two sets of images. We may therefore conclude, that the angles or sines of inflection and deflection, bear the same ratio to the angle or sine of incidence, and that they are equal to one another. My next object was to determine this ratio in one of these cases, and consequently in both; and it was very agreeable to find data for the solution of this problem in NEWTON'S measurements of the images and shadow; since this philosopher's well

known accuracy in such matters, besides the singular ingenuity of the methods he employed, made me more satisfied with these than any experiment I could make on the subject. In fig. 11. CS is the line perpendicular to the chart SU, and passing through the centre of the body, whose half is CD or SE; EB is parallel to CS, and AI a ray incident at D; ADB or EDI is the angle of incidence; EDR that of the red's deflection; EDV that of the violet's; and EDG that of the intermediate's. According to NEWTON,* CD was $\frac{1}{500}$th of an inch, DE 6 inches, SI $\frac{1}{108}$th of an inch, RV $\frac{1}{170}$th, and consequently RG $\frac{1}{340}$th; GS was $\frac{1}{76}$; whence the angles IDE, EDV, EDG, and EDR, will be found to be $4'$, $30''$; $5'$; $7'$, and $9'$, respectively. Now the natural sines of $4'$, $30''$; $5'$; $7'$, and $9'$, are as the numbers 1309, 1454, $2035\frac{1}{2}$, and 2617, which are as the sines of incidence, deflection, and inflection of the violet, green, and red. Thus the angles of flexion of the extreme and mean rays being given, those of the other rays are found by dividing the difference between 1454 and 2617 in the harmonical ratio: for then the red will be equal to $145\frac{3}{8}$; the orange $87\frac{9}{40}$; the yellow $155\frac{1}{15}$; the green $193\frac{5}{6}$; the blue $193\frac{5}{6}$; the indigo $129\frac{2}{9}$; and the violet $258\frac{4}{9}$; and by adding to the number 1454 the violet, and to their sum the indigo, and so on, we get the flexibility of the red, from 2617 to $2471\frac{5}{8}$; of the orange, from $2471\frac{5}{8}$ to $2384\frac{2}{5}$; of the yellow, from $2384\frac{2}{5}$ to $2229\frac{1}{3}$; of the green, from $2229\frac{1}{3}$ to $2035\frac{1}{2}$; of the blue, from $2035\frac{1}{2}$ to $1841\frac{2}{3}$; of the indigo, from $1841\frac{2}{3}$ to $1712\frac{4}{9}$; and of the violet, from $1712\frac{4}{9}$ to 1454: the common sine of incidence being 1309. It is therefore evident, that the flexibility of the red is not to that of the violet as the refrangi-

* Optics, Book III. Obs. 3.

bility of the violet to that of the red; and a little attention will convince us that we had no reason to expect the analogy should be kept up in this respect; for the refrangibility of the rays depends on the species of the refracting medium, and follows no general rule; whereas our calculation has been made concerning the action of the bending power at a certain distance, greater than that whereat the particles of media act on the rays in refracting them. It was observed, in the mathematical propositions prefixed to this paper, that the angle of flexion is less than that of incidence, when, in the case of inflection, the angle made by the ray and the body is acute, and when, in the case of deflection, that angle is obtuse; and when the ray is perpendicular or parallel, the angle of incidence vanishes in both cases. It is evident, therefore, that in both these situations of things the ratio of 1309 to 2036, being that of a less to a greater, will not enable us to find the angle of flexion, although it serves very well when the ray before inflection makes an obtuse, and before the deflection, an acute angle. I have therefore mentioned the angle made by the bent ray with the incident, which gives a general formula; for let the angle of incidence be I, and that which the bent ray makes with the incident B, then F being the angle of flexion, we have $F = B \pm I$; so that if $I = O$; $F = B$; or if the incident makes an obtuse angle with the body, in the case of deflection, and an acute in that of inflection, then $F = I - B$, and in the remaining case $F = I + B$.

These observations enable us to give a very short summary of optical science. When the particles of light pass at a certain distance from any body, a repulsive power drives them off; at a distance a little less, this power becomes attractive;

at a still less distance, it again becomes repulsive; and at the least distance, it becomes attractive as before; always acting in the same direction. These things hold whatever be the direction of the particles; but if, when produced, it passes through the body, then the nearest repulsive force drives the particles back, and the nearest attractive force either transmits them, or turns them out of their course during transmission. Farther, the particles differ in their dispositions to be acted upon by this power, in all these varieties of exertion; and those which are most strongly affected by its exertion in one case, are also most strongly affected by that exertion when varied; except in the cases of refraction, of which we before spoke; and these dispositions of the parts are in all the cases in the same harmonical ratio. Lastly, the cause of these different dispositions is the magnitude of the particles being various.

All that remains now to be done on this part of the subject is to explain one or two phænomena relating to reflexibility.

1. It has been remarked, that if we look at a candle, or other luminous body, with our eyes almost shut, bright streaks seem to dart upwards and downwards from it. NEWTON* explains this by refraction through the humours adhering to the eyelids. ROHAULT† and Mr. YOUNG‡ ascribe them to reflections. DES CARTES makes them arise from wrinkles on the eye's surface. DE LA HIRE from refraction through the moisture on the eyelids, as through a concave lens; and PRIESTLEY|| from inflection through the lashes. The truth of Sir ISAAC's explanation is obvious, because the streaks which dart from the top of the luminous body are formed

* *Lect. Opt. Sect. III. ad finem.*
† *Physica*, p. 249. CLARK's ed.
‡ Phil. Trans. 1793.
|| On Vision, Vol. II.

by the under eyelid, or at least by the moisture adhering to the under ciliary process, and those which appear from the bottom of the body, by the upper eyelid; which could not be, either if they were formed by reflection from the processes, or by inflection through the lashes.

I have, however, observed another kind of streaks, mottled with broken colours of all kinds, and formed by reflection from the moisture on the processes; in these the under streak corresponds to the under process, and *vice versa*: they may be formed by any polished body held in the proper position between the pupil and luminous body. The colours are very beautiful when made by the sun, and resemble, in form and irregularity of arrangement, some of the streaks made by large half-polished bodies, as described in Part II. of this paper.

2. The next object of attention is one of the greatest importance to our theory, namely, the formation of images by reflection: three things here require explanation, the number of the images, their colours, and their variations in point of size.

Obs. 15. I have uniformly found that no reflecting surface forms them, except it be curve, and (its surface) of a structure somewhat fibrous. A plain mirror, nor a concave, nor a convex one do not make them, unless they are of that structure; and, for the same reason, quicksilver, when held so as to reflect the light incident upon it, forms them not, but by *triturating* it, so as to divide it into small particles, and by placing these in the beam of the sun's light, each particle formed an image, with the colours in the regular order and very bright: on holding a cylinder in the rays, and observing the lengths of the images, I found that if the curvature was increased, the

images were also increased in size, being more distended, and highly coloured. These things immediately suggest the explanation. Each of the small fibres forms an image, which, from the different reflexibility of the rays, is divided into the seven primary colours. But why does not a plain mirror form *one* of these upon the same principles? In fig. 12. let AE be the curve surface of a very convex mirror, that is of a small fibre; GC a ray reflected by the small surface DC; it will be separated into CI red, and CK violet, by the unequal action of FC on its parts. But if DC is continued to L in a straight line, then LC's sphere of reflection extending a little way beyond it, to KC, the part nearest to C, and not to IC, will drive KC and also the indigo and part of the blue nearer to the perpendicular; then IC being within LC's sphere of inflection, will, together with the orange, yellow, and part of the green, be brought nearer to KC; so that IC and KC will both be brought to an angle equal to that of incidence, and will be reflected in a parallel white beam. If LC is removed a little, or the surface becomes more convex, IC is attracted, and KC repelled, but not so much as to reduce them to parallelism and whiteness, an image being formed narrower and less coloured than when LC is moved so far round that KC is attracted, and IC deflected or repelled. If LC is moved round so that the mirror is concave, then KC is repelled, and IC attracted, as before, unless the curvature be considerable; and then KC and IC are both repelled, and an image formed in the *caustic* by reflection. In Obs. 3. we found that certain irregularities in the surface of the reflector caused the images to be in the inverted order of colours. How does this happen? In fig. 13. let *gf*, *fe*, *er*, *ri*, and *ih*, represent the sections of the con-

vex fibres on the surface of the reflector, and let the ray AB be reflected from *ef*, separated into B*r* red, and B*v* violet; then if AB was so inclined to *ef*, that B*r* and B*v* fell upon *er*, the side of the fibre next to *ef*, and a little larger than *ef*, it is evident that B*v* will be reflected into *v*V, and B*r* into *r*R, and an image VR will be formed, having the violet outermost and the red innermost, the intermediate colours being in their order, from V to R. Lastly, it is evident that the greater the angle of incidence is, the longer will be the image, and the farther separated its colours; for which reason the farther the images are from the shadow, the less dilated and coloured will they be. Nor will they have the same appearance at all distances from the point of incidence; very near it, they will be all in the form of fringes across the streak, the breadth being greater than the length (if I may use the expression), but as we recede from it, they will become distended, as before described, the length increasing faster than the breadth, and at one point or distance they will be just as long as broad; all which agrees with experiment; and it is needless to show by particular demonstration, the manner in which one image is divided from another, the reason obviously being the manner in which the fibres on the reflecting surface are arranged and inclined to one another.

3. A number of phænomena, involved in that of the images, are explicable by what has been said on them. If a piece of metal be scratched, and then exposed in the sunshine, a number of broken colours will be formed by the scratches, as may be seen either by letting them fall on the eye, or by receiving them on a white object. This is evidently owing to the different reflexibility of the rays incident on the scratches, which

are so many irregular specula, of great curvature; the images are therefore distorted and broken, just as a candle, &c. appears broken and coloured when viewed through a piece of irregular crystal, such as the bottom of a wine glass. If we look attentively at any object exposed in the light of the sun, provided it be not polished, we shall see its surface mottled with various points of colours, from the specular nature of its minute particles. If we look towards the sun, with a hat on our head, held down, so that the sun's direct light may not fall on our eyes, but on the hairs of the hat, and be reflected, we shall see a variety of lively colours darting in all directions from those hairs; and we may easily satisfy ourselves that they are not the consequence of flexion, by trying the same thing with unpolished threads, in which case they do not appear, provided the threads be not very small. In the same manner we may account for the colours of spider webs, of different cloths which change their colours when their position is altered, and of some fossils which appear of different streaks of colours when held in the light, such as the fire marble of Saxony, &c. All these bodies having surfaces of a fibrous structure, each fibre reflects and decompounds the rays.

4. The consideration of the foregoing phænomena inclined me to think, that upon the principles which have been laid down, the colours of natural bodies may be explained. The celebrated discovery of NEWTON, that these depend on the thickness of their parts, is degraded by a comparison with his hypothesis of the fits of rays and waves of æther. Delighted and astonished by the former, we gladly turn from the latter; and unwilling to involve in the smoke of unintelligible theory so fair a fabric, founded on strict induction, we wish to find

some continuation of experiments and observations which may relieve us from the necessity of the supposition. My speculations on this subject have by no means been completed, as I have not yet finished the demonstrations and experiments into which it has engaged me to enter; but, in order to complete my plan, I shall offer a few hints on the subject. The parts of light are affirmed, in Prop. III. Book I. Part I. of the Optics, to be different in reflexibility; that is, according to the author's definition, in disposition to be turned back, and not transmitted at the confines of two transparent media. That the demonstration involves a logical error appears pretty evident. When the rays, by refraction through the base of the prism used in the experiment, are separated into their parts, these become divergent, the violet and red emerging at very different angles, and these were also incident on the base at different angles, from the refraction of the side at which they entered; when, therefore, the prism is moved round on its axis, as described in the proposition, the base is nearest the violet, from the position of the rays by refraction, and meets it first; so that the violet being reflected as soon as it meets the base, it is reflected before any of the other rays, not from a different disposition to be so, but merely from its different refrangibility; although then this experiment is a complete proof of the different refrangibility of the rays, it proves nothing else; and indeed an experiment will convince us, that the rays all have the same disposition to be reflected, provided the angle of incidence be the same. For I held a prism vertically, and let the spectrum of another prism be reflected by the base of the former, so that the rays had all the same angle of incidence; then turning round the vertical prism on its axis, when one sort of rays

was transmitted or reflected, all were transmitted or reflected. We cannot therefore apply the different reflexibility of light, to the explanation of the colours of bodies, since this property has no existence. But we have shewn that the rays differ in *reflexibility*, taking the word in the new sense, as explained above; let us see whether this principle will not solve the important problem. It is evident that the particles of bodies are specular. Now I take the colours of bodies to depend, not on the size, but on the position of these particles, or at least on only the size in as far as it influences their position; an idea perfectly familiar to mathematicians.

Obs. 16. In making some of the experiments, which I related above on the reflexibility of light, I observed, among the regular images made by most of the pins which I used, one or two all of the same colour, as red, blue, *&c.* and when the pin was moved these moved also, becoming of other colours in regular order, like the rest; which shows plainly that their being of one colour at first was owing to some fibre in the surface jutting out, or rather to several of these, which stopped the red and all the rest but the blue of several images, or the blue and all the rest but the red. Farther, I produced several regular images by two or three very small pins, and with considerable trouble I at last contrived to place them in such a position as that one blue colour of considerable size might be produced, then a red, and so on, by altering the posture of the pins; now, whether the posture or the size be altered it matters not, for the one affects the other. Is it not evident that this experiment, and the conclusion to which it evidently leads, may be transferred to the colours of natural bodies as seen by reflection? for the parts being specular and

spherical, each will form an image of the luminous body; and by the position of the sides of the neighbouring ones, any six of the colours may be stopped, while the seventh emerges; and if this happens in one part, it will happen in all, since that the texture and size of the parts is the same throughout, has never been called in question. But it will be asked, how are the particles to reflect a mixture of different colours? We answer, that a particle having its sides concave, and front convex, will produce the effect; for the colours will be thus mixed in a proportion determined by the position of the others. How can whiteness and blackness be produced? If the particles be large, then the whole light incident on each will be reflected and separated, and all the images being compounded and mixed together, a confused sensation, or a sensation of white, will be the result. For the parts being transparent, and the images formed by the convex surface of the second row of particles, these will be larger in proportion to the thickness of the particles, or plates through which they have to pass before they meet with obstruction, and consequently will not be stopped by other particles; and in like manner the colour will be red if the particles are a little less, and so on. If the particles be very small, the light will be separated into images also small, with which, and with one another, the particles interfering, the light by many reflections and obstructions will be totally lost. How do bodies appear of their proper colours though no luminous body be shining, whose image may be formed by a reflection? They reflect images of the clouds, which reflect the sun's white light; for if we hold between our eye and a hole in the window, illuminated by the light of the clouds, a reflecting body, as a pin, &c. coloured images are

formed of the hole distended like those of the sun, as I have often found; and the same holds of inflection. Why does cutting a body to pieces not alter its colours? This only changes the position of masses of particles, not of the particles themselves; but if by bruising them we change their magnitude and position, we change also their colour; thus the leaves of vegetables bruised in a mortar, many paints powdered, &c. Why do many bodies change colours when viewed in different positions? Because they reflect two colours, or more, of each image to different quarters; and it matters not whether their position with respect to us or our position with respect to them be changed. How do bodies appear coloured by transmitted light? Because the foregoing reasonings apply also to the flexion of the rays in their passage through the parts of bodies. These observations appear to me to furnish a very simple solution of the problem. I shall endeavour, in a future communication, to confirm what has been said, by other remarks and experiments; for it would be tedious, and perhaps superfluous, to illustrate what has been said by figures and demonstrations.*

Pursuant to these remarks, it will not be difficult to account for the rings of colours of thin plates by reflection, as we before did those of thick plates by flexion;† indeed those formed in the experiment of the two lenses, supposed by NEWTON to

* It is obvious that the different refrangibility of the rays will not account for the bright and distinct colours of bodies: if the refracting angle of a prism be continually diminished, till, for example, it is equal to one of a minute, the refraction will produce no sensible colours; indeed almost every piece of plane glass has its sides in a small degree inclined to one another, and yet no colours are formed; much less then will refraction through the infinitely smaller parts of bodies, produce separation of the rays.

† Page 260 of this paper.

be owing to the plates of air between them, appear to have a different cause, as may be without much reasoning gathered from the curious experiments of the Abbé MAZEAS,* and even from one or two of Sir ISAAC's own, in which he supposes some medium more subtile than air to be between the glasses.† But at present I forbear entering into the subject any farther: this paper has been already extended to a greater length than was at first intended. And I hasten to conclude, by a short summary of Propositions, containing the principal things which have been demonstrated in the course of it.

Prop. I. The angles of inflection and deflection are equal, at equal incidences.

Prop. II. The sine of inflection is to that of incidence in a given ratio (which is determined in the paper.)

Prop. III. The sun's light consists of parts which differ in degree of inflexibility and deflexibility, those which are most refrangible being least flexible.

Prop. IV. The flexibilities of the rays are inversely as their refrangibilities; and the spectrum by flexion is divided by the harmonical ratio, like the spectrum by refraction.

Prop. V. The angle of reflection is not equal to that of incidence, except in particular (though common) combinations of circumstances, and in the mean rays of the spectrum.

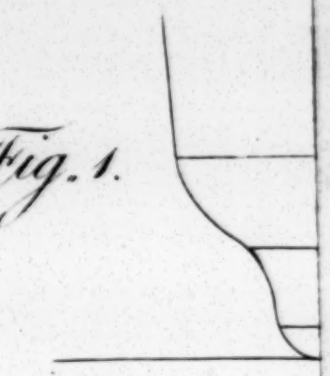
Prop. VI. The rays which are most refrangible are least reflexible, or make the least angle of reflection.

Prop. VII. The reflexibilities of the different rays are inversely as their refrangibilities, and the spectrum by reflection is divided in the harmonical ratio, like that by refraction.

* *Mém. de l'Académie pour l'année* 1738.

† Optics, Book II. Part I. Obs. 10 and 11.

Fig. 1.



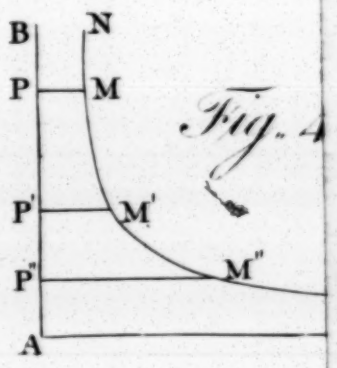


Fig. 4.

Fig. 7.



Fig. 11.



Fig. 1.

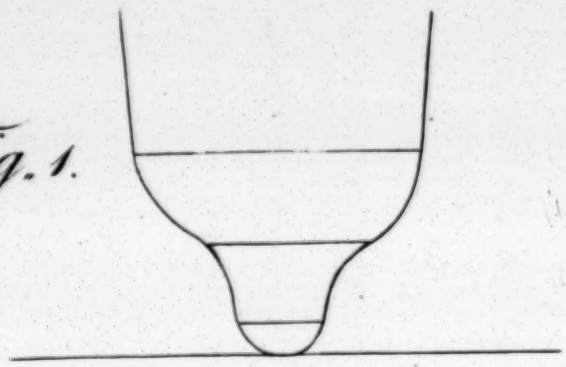


Fig. 2.

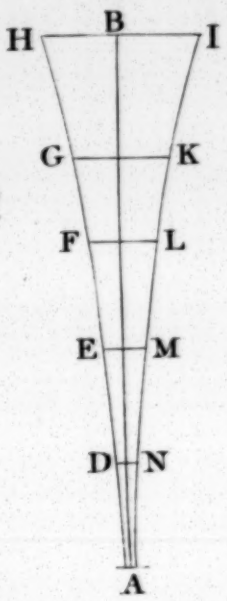


Fig. 4.

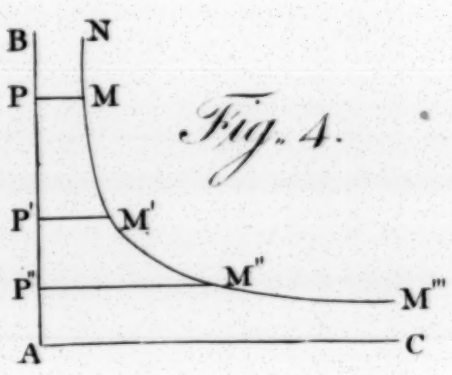
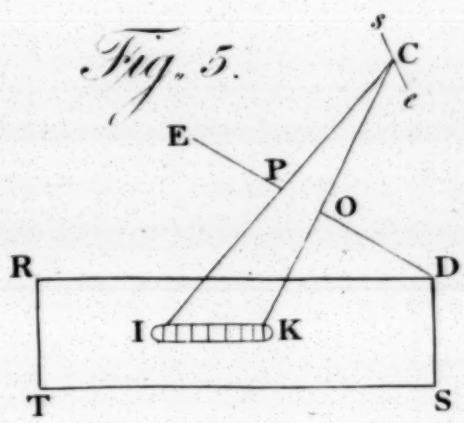


Fig. 5.



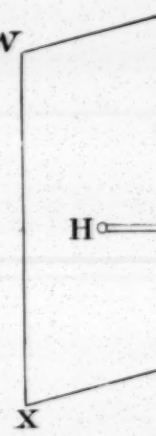


Fig. 7.

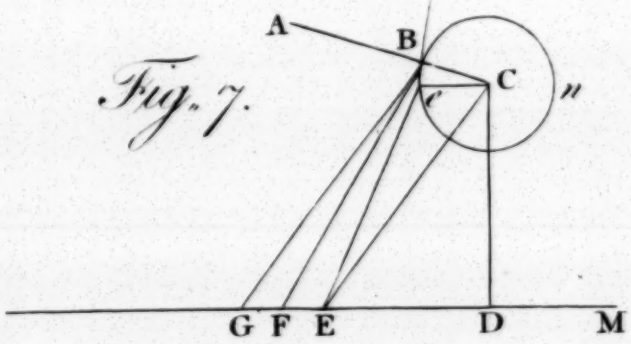


Fig. 8.

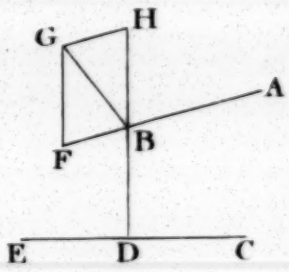


Fig. 11.

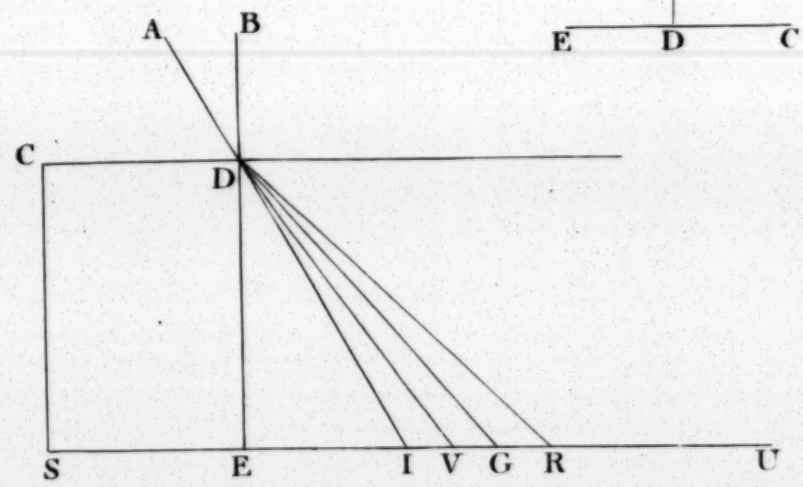
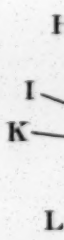
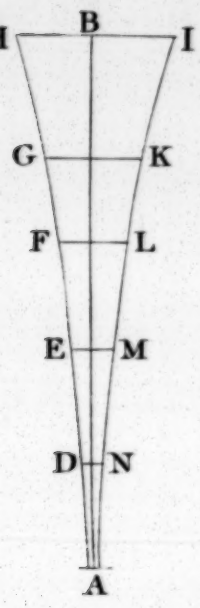


Fig. 12.





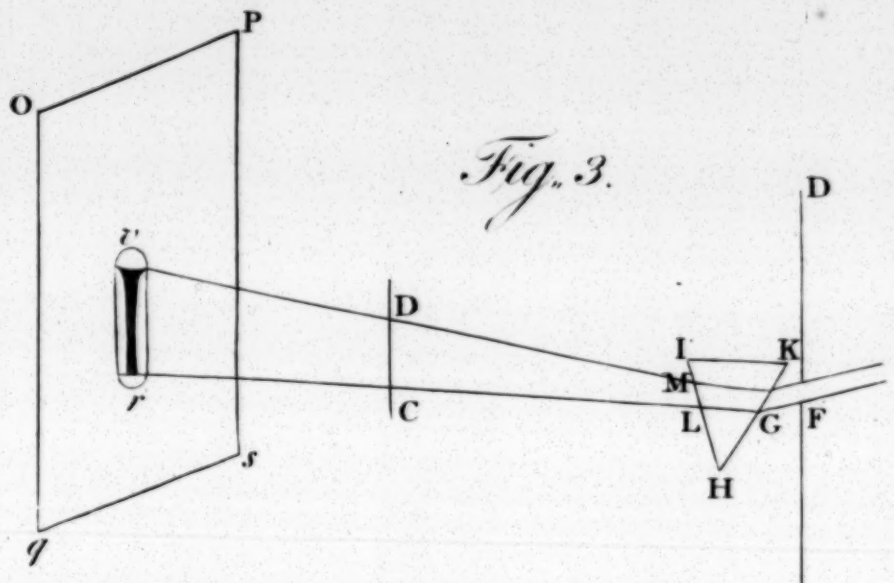


Fig. 3.

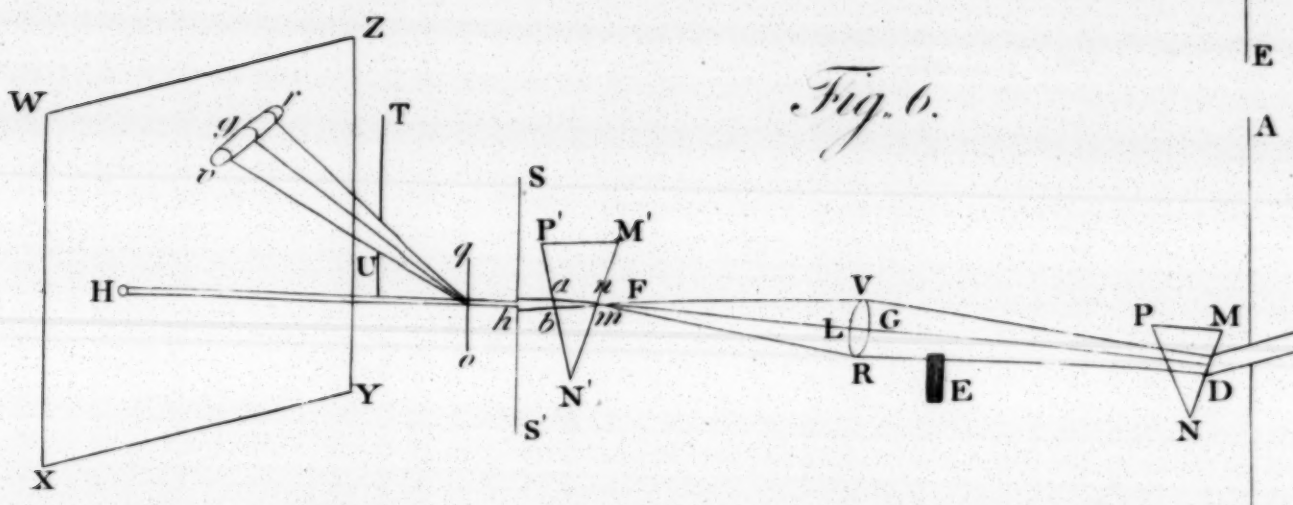


Fig. 6.

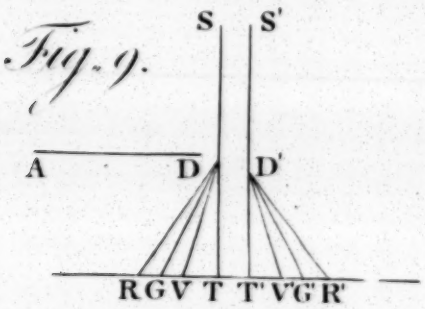


Fig. 9.



Fig. 10.

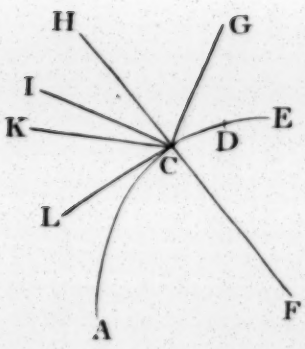


Fig. 12.

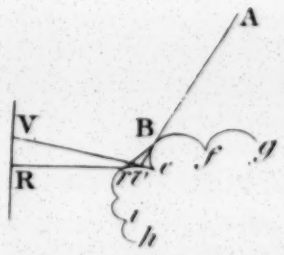


Fig. 13.

Prop. VIII. The sines of reflection of the different rays are in given ratios to those of incidence (which are determined in the paper.)

Prop. IX. The ratio of the sizes of the different parts of light are found.

Prop. X. The colours of natural bodies are found to depend on the different reflexibilities of the rays, and sometimes on their flexibilities.

Prop. XI. The rays of light are reflected, refracted, inflected, and deflected, by one and the same power, variously exerted in different circumstances.

ERRATA.

Page 52, line 26, *dele* practically.

Page 61, line 5, *for* in general is, *read* is general, being.

Page 65, line 8, *for* point X, *read* horizontal line drawn through the point X parallel to the axis of motion.

Page 77, lines 1 and 9, *for* WGS, *read* UGS; and line 24, *for* WGO, *read* UGO.

Page 78, line 2, *for* VW, *read* VU.

Page 85, line 26, *for* B, *read* R.

Page 91, last line, *for* QA, *read* NF.

Page 96, last line, *for* prop. iii. *read* prop. ii.

Page 97, line 18, *for* GZ, *read* ΓZ.

Page 100, line 12, *for* horizontal line, *read* indefinite horizontal line.

Page 107, line 3 and 4, *dele* HD = HA.

Page 115, line 5, *for* AB — PX, *read* WP — PX, fig. 11. and 28.

Page 124, line 11, *for* is, *read* are.

Note to be added to page 104, last line, to the word "inquiry."

The following remark on the propositions and demonstrations of APOLLONIUS PERGÆUS, equally, or rather more applicable to those of ARCHIMEDES, is extracted from Dr. WALLIS's Algebra.

"Et quidem meritò censeri posset ille, magnus geometra, et prodigiosæ, tum phan-
"tasiæ tum memoriæ vir, si possibile putemus ut potuerit ille propositiones et demon-
"strationes perplexas, eo ordine quo ad nos perveniunt invenire, absque cujusmodi
"aliquâ *inveniendi arte* qualis est quam nos algebram dicimus."

Dr. WALLIS's Algebra, cap. LXXVI.

Page 124, line 26, note to the words "first applied."

PERE PARDIES and Chevalier RENAUD published some partial observations on the theory of naval architecture rather before this period: but the treatise of M. L'HOSTE seems to be the first work in which this subject is considered systematically, and at length.

Page 127, line 8, *for* whatever may have been, *read* whatever may be.

Page 135, line 7, insert *the Rev.* before Nevil.

Page 202, lines 28, 30, and 31, *for* w^1, w^2, w^3, *read* ω^1, ω^2, ω^3.

Page 205, line 27, *for* w, *read* ω.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1795.

1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Jan. 1	22	8 0	22	47	30,17	77		ENE	1	Foggy.
	26	2 0	26	47	30,18	73		ENE	1	Foggy.
2	20	8 0	21	46	30,23	77		NE	1	Foggy.
	23,5	2 0	23,5	48	30,26	76		NE	1	Foggy.
3	13,5	8 0	14	44	30,35	75		E	1	Cloudy.
	20	2 0	20	44,5	30,37	73		S	1	Hazy.
4	13	8 0	13,5	42	30,47	73		E	1	Cloudy.
	23,5	2 0	23	44	30,44	73		E	1	Foggy.
5	19,5	8 0	22	42	30,31	75		S	1	Cloudy.
	33	2 0	32	45	30,26	76		SW	1	Fair.
6	26,5	8 0	28,5	43	30,26	75		W	1	Cloudy.
	35	2 0	32	47	30,28	77		W	1	Cloudy.
7	31	8 0	33	46	30,36	83		E	1	Cloudy.
	35	2 0	33	46,5	30,31	83		ESE	1	Cloudy.
8	32,5	8 0	33,5	45	30,10	79		E	1	Cloudy.
	36	2 0	36	47	30,01	74		ENE	1	Cloudy.
9	29	8 0	31	46	30,01	79		WNW	1	Cloudy.
	38	2 0	37	48	30,08	80		WNW	1	Cloudy.
10	29	8 0	29,5	46,5	30,41	77		NE	1	Cloudy.
	34,5	2 0	34	49	30,45	73		NE	1	Fine.
11	23	8 0	23,5	47	30,46	77		NE	1	Fair.
	31	2 0	31	48	30,44	75		WNW	1	Hazy.
12	21	8 0	22	46	30,40	75		WNW	1	Cloudy.
	33	2 0	29	48	30,38	74		WNW	1	Cloudy.
13	31	8 0	32	46	30,29	79		E	1	Snow.
	34	2 0	32	49	30,22	72		E	1	Fine.
14	22,5	8 0	23	43	30,18	73		E	1	Fine.
	29	2 0	28	44,5	30,18	66		ENE	1	Cloudy.
15	27	8 0	28	45	30,10	77		NE	2	Cloudy.
	30	2 0	25,5	46	30,14	66		NE	2	Cloudy.
16	21	8 0	22	42	29,91	67		NNE	2	Cloudy.
	29	2 0	29	46	29,78	66		NNE	2	Cloudy.

METEOROLOGICAL JOURNAL

for January, 1795.

1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Jan. 17	22	8 0	24	42	29,74	76		NE	1	Snow.
	28	2 0	26	44,5	29,76	72		NE	1	Cloudy.
18	25	8 0	26	42	29,60	74		NE	1	Snow.
	28,5	2 0	28	45	29,58	72		NNE	2	Cloudy.
19	20,5	8 0	22	41,5	29,65	77		NE	1	Cloudy.
	28,5	2 0	28,5	44	29,69	69		NE	1	Cloudy.
20	14	8 0	17	39	29,72	71		NE	1	Cloudy.
	23,5	2 0	23	40	29,71	70		NE	1	Cloudy.
21	19	8 0	19	38,5	29,77	74		NE	1	Snow.
	23	2 0	23	40	29,81	72		NE	1	Cloudy.
22	17	8 0	18	38,5	29,85	74		NNE	1	Cloudy.
	23	2 0	22,5	40,5	29,82	72		NNE	1	Hazy.
23	13	8 0	16	38	29,69	73		NE	1	Cloudy.
	20	2 0	20	40	29,69	70		NE	1	Fair.
24	14,5	8 0	21,5	38	29,94	73		NE	1	Cloudy.
	25	2 0	25	41	30,00	73		NW	1	Cloudy.
25	7	8 0	8	37	30,07	72		E	1	Foggy.
	21	2 0	21	39	30,08	71		NW	1	Hazy.
26	17,5	8 0	19	36	29,75	72		ESE	2	Cloudy.
	38	2 0	22	38	29,52	74		ESE	2	Cloudy.
27	40	8 0	43	39	29,19	89	0,183	SSW	1	Cloudy.
	46	2 0	46	43	29,18	90		SSW	1	Cloudy.
28	38	8 0	38	42	29,16	92	0,243	NE	1	Rain.
	38	2 0	34	44	29,44	88		NE	2	Cloudy.
29	24	8 0	24	42	30,02	75	0,050	NNE	1	Fair.
	29	2 0	29	43,5	30,01	76		NNE	1	Fine.
30	21	8 0	23	42	30,13	83		NNE	1	Foggy.
	26,5	2 0	26,5	44	30,16	80		N by E	1	Cloudy.
31	19,5	8 0	21	41	30,13	81		ENE	1	Foggy.
	33	2 0	33	44	30,06	72		ESE	1	Cloudy.

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1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Feb. 1	32	7 0	33	42,5	29,65	86		ESE	1	Foggy.
	41	2 0	36	45	29,30	90		ESE	1	Rain.
2	32	7 0	33	43	29,29	73	0,330	W	2	Cloudy.
	35	2 0	34,5	45	29,32	75		WNW	2	Cloudy.
3	30	7 0	26	44	29,22	81		SW	2	Snow.
	35,5	2 0	35	46	29,12	74		WSW	2	Cloudy.
4	27	7 0	27	42	29,08	75		WSW	1	Cloudy.
	35,5	2 0	35,5	47	29,25	64		W	2	Fine.
5	27	7 0	30	44	29,60	75		WNW	1	Cloudy.
	36,5	2 0	36,5	47	29,72	70		WNW	1	Fine.
6	26	7 0	27	44,5	29,98	76		WNW	1	Cloudy.
	34	2 0	34	47	29,95	73		E	1	Cloudy.
7	31	7 0	31,5	44	29,55	75		E	1	Cloudy.
	35,5	2 0	35	48	29,46	73		E	1	Fair.
8	33	7 0	39	46	29,27	88		SE	1	Cloudy.
	44	2 0	43,5	49	29,27	89		S	2	Cloudy.
9	40	7 0	45	48	29,22	91	0,160	S	2	Rain.
	51	2 0	50	50,5	29,17	90		S b. W	2	Rain.
10	48	7 0	50,5	50	29,16	86	0,505	S	2	Rain.
	51	2 0	51	53	29,04	81		S	2	Cloudy.
11	47	7 0	48	53	29,16	82	0,080	SSW	2	Cloudy.
	51	2 0	51	55	29,10	73		SSW	2	Fair.
12	45	7 0	45	54	29,10	78		WSW	2	Cloudy.
	44	2 0	44	54	29,27	75		SW	2	Cloudy.
13	31	7 0	32	51	29,24	88		NW	2	Snow.
	37,5	2 0	37	52	29,42	69		NE	2	Cloudy.
14	30	7 0	30	51	29,97	72	0,060	NE	1	Cloudy.
	37	2 0	37	53	30,10	70		NE	1	Fine.
15	28	7 0	28	50	30,47	77		NE	1	Fair.
	38	2 0	38	51,5	30,51	73		W	1	Hazy.
16	34	7 0	34	50	30,66	84		NNE	1	Cloudy.
	37	2 0	36	50	30,68	79		E	1	Cloudy.

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1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Feb. 17	28	7 0	29	49	30,60	82		E	1	Cloudy.
	35	2 0	33	49	30,50	81		E	1	Cloudy.
18	28	7 0	29	47	30,39	67		E	2	Cloudy.
	32	2 0	31	48	30,32	64		E	2	Cloudy.
19	26	7 0	27	44	30,08	66		NE	2	Cloudy.
	30	2 0	28	44,5	29,99	67		NE	2	Snow.
20	24	7 0	25	43	29,90	66		NE	2	Cloudy.
	27	2 0	26	45	29,78	74		NE	2	Snow.
21	24	7 0	25	42	29,73	70		E	1	Cloudy.
	32	2 0	31	45	29,73	70		ESE	1	Fair.
22	32	7 0	32	42,5	29,55	73		E	1	Cloudy.
	37	2 0	37	45,5	29,45	74		E	2	Cloudy.
23	35,5	7 0	39	45	29,52	88	0,035	SSW	2	Cloudy.
	46	2 0	44	48,5	29,57	80		S	2	Cloudy.
24	37	7 0	39	47	29,57	80		SSW	1	Cloudy.
	44	2 0	44	50	29,60	77		WNW	2	Cloudy.
25	38,5	7 0	39	48	29,48	83		SSW	1	Cloudy.
	46	2 0	46	51	29,38	74		SSE	2	Cloudy.
26	38	7 0	38	48	29,18	83		ESE	1	Cloudy.
	43	2 0	43	51	29,05	78		ESE	2	Cloudy.
27	37	7 0	37,5	49	29,06	82	0,085	NE	2	Cloudy.
	41	2 0	41	51	29,18	70		NNE	1	Cloudy.
28	31	7 0	31	49	29,41	78		E	1	Snow.
	34	2 0	34	50	29,23	80		E	1	Snow.

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1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Mar. 1	24	7 0	25	47	29,72	71	0,160	N	1	Fine.
	34	2 0	33,5	50,5	29,90	67		NW	1	Fine.
2	28	7 0	34	48	29,97	73		SW	1	Cloudy.
	38	2 0	36	49	29,86	73		SE	2	Cloudy.
3	34	7 0	34,5	48	30,19	74		E	1	Cloudy.
	37	2 0	36,5	49	30,23	72		W	1	Cloudy.
4	34	7 0	38	48	30,00	85		SSW	1	Cloudy.
	49	2 0	48,5	53	29,92	79		W	1	Cloudy.
5	40,5	7 0	44	50	29,84	85	0,060	S	1	Rain.
	51,5	2 0	50	53	29,68	71		S	2	Cloudy.
6	46	7 0	48	52	29,15	82	0,031	WSW	2	Cloudy.
	50,5	2 0	48	56	29,34	68		W	2	Cloudy.
7	43,5	7 0	43,5	52,5	29,57	72		NW	2	Cloudy.
	45	2 0	44	55	29,81	70		NW	2	Cloudy.
8	33	7 0	35	52	30,28	69		N	2	Fine.
	44	2 0	43,5	55	30,35	61		NNE	2	Fair.
9	37	7 0	38	52,5	30,16	78		S	1	Cloudy.
	46	2 0	43	54	29,95	80		S	1	Rain.
10	38	7 0	39	52	29,59	81	0,268	SW	1	Fair.
	48	2 0	47	55	29,46	68		SSW	2	Cloudy.
11	37	7 0	37	52	29,22	78	0,141	WSW	1	Fair.
	47	2 0	44	55	29,26	65		W	1	Fair.
12	34	7 0	35	52	29,35	76		WSW	1	Cloudy.
	45	2 0	44	55	29,28	72		SE	2	Cloudy.
13	33,5	7 0	35	53	29,14	80	0,295	NE	1	Cloudy.
	38	2 0	37	53	29,25	74		NE	1	Cloudy.
14	32	7 0	33	50	29,64	73		E	1	Cloudy.
	39	2 0	38,5	52	29,60	69		E	2	Cloudy.
15	33	7 0	36	50	29,54	80	0,116	WSW	1	Cloudy.
	45	2 0	44,5	52,5	29,58	63		SW	1	Fair.
16	33	7 0	38	50	29,13	85	0,238	E	1	Rain.
	46	2 0	46	53	29,02	85		SSE	1	Rain.

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		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Mar. 17	36	7 0	36	51	29,40	74	0,389	NW	1	Cloudy.
	38	2 0	36,5	52	29,40	73		NW	1	Cloudy.
18	34	7 0	35,5	50	29,56	78		NE	2	Cloudy.
	38	2 0	37	50,5	29,74	71		NE	2	Cloudy.
19	30	7 0	31	49,5	30,14	75		NE	1	Cloudy.
	43	2 0	43	51	30,17	65		W	1	Fair.
20	35	7 0	38	50	30,21	73		WNW	1	Fair.
	48,5	2 0	48,5	52	30,27	67		NW	1	Cloudy.
21	34	7 0	36	51	30,22	76		W	1	Fair.
	50	2 0	50	54	30,08	64		WSW	1	Fair.
22	39	7 0	40	52	30,07	76		NW	1	Cloudy.
	49	2 0	49	55,5	30,11	63		NNE	1	Cloudy.
23	34	7 0	35	52	30,15	77		W	1	Hazy.
	45,5	2 0	45	55	30,10	71		W	1	Hazy.
24	38	7 0	39,5	53	30,00	75		WSW	1	Cloudy.
	50	2 0	50	55	29,93	66		SW	1	Cloudy.
25	37	7 0	38	53	29,82	75		S	1	Fair.
	49	2 0	48	55	29,78	63		S	1	Cloudy.
26	37	7 0	39	53	29,81	7[illegible]		NE	1	Fair.
	50,5	2 0	49	57	29,84	63		NE	1	Cloudy.
27	35	7 0	38	53	29,90	72	0,046	NE	1	Cloudy.
	46	2 0	45	57	29,90	67		NE	1	Fair.
28	34	7 0	36	53	30,00	74		ENE	1	Cloudy.
	48	2 0	48	57	30,03	69		ENE	1	Fair.
29	35	7 0	37	54	30,02	76		E	1	Cloudy.
	50	2 0	49,5	57	30,02	70		E	1	Fine.
30	38	7 0	39	54,5	30,00	75		SSE	1	Cloudy.
	51	2 0	51	58	29,98	65		S by E	2	Fair.
31	38	7 0	41	55	29,95	77		ESE	1	Fine.
	54,5	2 0	54,5	59	29,90	56		ESE	1	Fine.

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1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
April 1	36,5	7 0	37,5	54,5	29,93	76		E	1	Cloudy.
	48	2 0	47	59	29,91	74		ESE	1	Fair.
2	36	7 0	37	54	29,87	78		ESE	1	Cloudy.
	48	2 0	45	57	29,86	74		ESE	1	Fair.
3	38	7 0	39	55	29,82	80		ESE	1	Cloudy.
	51,5	2 0	50	58	29,83	70		ESE	1	Fair.
4	37	7 0	38,5	55	30,08	70		NE	1	Cloudy.
	44	2 0	42	56	30,10	70		NE	2	Cloudy.
5	39	7 0	39	54	30,09	74		NE	1	Cloudy.
	44	2 0	42,5	56	30,08	72		NE	1	Cloudy.
6	38,5	7 0	40	55	30,06	75		NE	1	Cloudy.
	43	2 0	43	56	30,04	76		NE	1	Rain.
7	37,5	7 0	39	54	29,95	74	0,033	NE	1	Cloudy.
	43	2 0	43	56	29,89	71		NE	1	Cloudy.
8	37,5	7 0	38	53	29,84	72		NNW	1	Cloudy.
	47,5	2 0	44,5	57	29,80	69		NE	1	Cloudy.
9	36	7 0	38	54	29,84	76		NE	1	Cloudy.
	46	2 0	45	55,5	29,87	75		NE	1	Cloudy.
10	39	7 0	40	53,5	30,00	79		NE	1	Cloudy.
	58	2 0	56	58	30,00	71		E	1	Fine.
11	42	7 0	44	56	30,04	79		NE	1	Cloudy.
	60	2 0	60	59	30,04	71		E	1	Fine
12	47	7 0	47,5	57	30,02	79		E	1	Cloudy.
	56	2 0	55	59	30,02	70		NE	1	Cloudy.
13	42,5	7 0	44	57	30,00	70		NE	1	Cloudy.
	49	2 0	49	58	30,07	65		NE	1	Cloudy.
14	39	7 0	41,5	57	30,22	72		E	1	Fair.
	61	2 0	61	60	30,20	61		S	1	Hazy.
15	50	7 0	51	58	30,15	75		SW	1	Cloudy.
	58,5	2 0	58	59,5	30,08	71		SSW	1	Cloudy.
16	44	7 0	47	57	29,93	72		S b. W	1	Fair.
	59,5	2 0	58,5	60	29,88	65		SSW	1	Cloudy.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Apr. 17	44,5	7 0	46	58	29,88	70		SW	1	Fair.
	56	2 0	56	60,5	29,88	54		WSW	1	Fair.
18	43	7 0	45	57	29,82	70		SSW	2	Fair.
	55	2 0	54	58	29,66	62		S	2	Cloudy.
19	46	7 0	47	57	29,41	71	0,033	SSE	2	Fair.
	56	2 0	56	58	29,38	61		SE	2	Fair.
20	46	7 0	46	57	29,36	74	0,034	E	1	Rain.
	55	2 0	55	59	29,34	66		S	1	Cloudy.
21	43	7 0	45	57,5	29,35	72	0,071	S	2	Fair.
	56	2 0	55	59	29,34	61		S	2	Cloudy.
22	41	7 0	42	58	29,35	73	0,032	SSW	1	Hazy.
	57	2 0	56	59,5	29,35	58		W	1	Fair.
23	39	7 0	41	58	29,49	68		SW	1	Fine.
	56	2 0	49	59	29,49	69		W	1	Cloudy.
24	44	7 0	46	58	29,48	70	0,186	NE	1	Cloudy.
	58	2 0	58	61	29,60	58		W	2	Fair.
25	48	7 0	49	58	29,40	75	0,037	SW	2	Cloudy.
	59	2 0	55	60	29,50	66		SW	2	Cloudy.
26	45	7 0	47	58	29,73	67		WSW	2	Fair.
	58	2 0	57,5	61,5	29,87	60		WSW	2	Fair.
27	47	7 0	48	58	29,91	69		SSW	2	Cloudy.
	57,5	2 0	56	60	29,91	65		SSW	2	Cloudy.
28	48	7 0	48	58,5	29,60	69		S	2	Cloudy. [much wind last night.
	55	2 0	52	60	29,38	69		S	2	Cloudy.
29	43	7 0	45,5	58	29,59	70	0,071	SW	2	Cloudy.
	57,5	2 0	56	60	29,64	60		SW	2	Fair.
30	48	7 0	52	58,5	29,48	68		S	2	Cloudy.
	57	2 0	56	59,5	29,53	62		SSW	2	Fair.

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1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
May 1	44	7 0	47	58	29,76	68	0,035	SSW	2	Fine.
	61	2 0	61	62	29,81	54		SSW	2	Fine
2	44	7 0	49	59,5	29,99	69		S	2	Hazy.
	60	2 0	60	62	30,04	60		NE	2	Hazy.
3	40	7 0	43	59	30,28	70		ENE	1	Hazy.
	63	2 0	63	63	30,35	60		NE	1	Fine.
4	44	7 0	46	60	30;38	71		NE	1	Hazy.
	64	2 0	63	63	30,36	60		NE	1	Hazy.
5	48	7 0	53	61	30,34	71		SSW	1	Fair.
	72,5	2 0	72	63	30,30	60		NE	1	Fine.
6	55	7 0	56	62,5	30,37	70		NE	1	Cloudy.
	64,5	2 0	63	63	30,37	64		NE	1	Cloudy.
7	46	7 0	49	61	30,33	68		NE	1	Hazy.
	68,5	2 0	68	63	30,25	54		W	1	Fine.
8	54	7 0	55	63	30,23	62		NE	1	Fine.
	56	2 0	56	63	30,38	55		NE	1	Fine.
9	37	7 0	40,5	61	30,48	60		NE	1	Fine.
	59	2 0	58	61	30,37	55		W	1	Fine.
10	47	7 0	51	60,5	30,19	65		W	1	Fine.
	70	2 0	70	63	30,08	56		W	2	Fine.
11	48	7 0	50	61	29,92	63	0,030	NW	2	Fair.
	58	2 0	57,5	62,5	29,98	59		NW	2	Fair.
12	39	7 0	42	59	30,30	62	0,038	NE	2	Fair.
	56	2 0	56	61	30,29	57		N	2	Fair.
13	47	7 0	49	59	30,17	62		NW	2	Cloudy.
	58	2 0	56	59	30,13	61		NNW	1	Cloudy.
14	43	7 0	45	57	30,22	63		NE	1	Cloudy.
	56	2 0	56	57	30,22	60		ESE	1	Cloudy.
15	44	7 0	47	57,5	29,98	68	0,028	W	1	Cloudy.
	58	2 0	57,5	58	29,87	60		WSW	2	Cloudy.
16	41	7 0	43,5	57,5	29,94	67		NW	2	Cloudy.
	55	2 0	55	58	29,97	57		WNW	1	Hazy.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
May 17	45	7 0	46	57	29,98	61		E	1	Fair.
	63,5	2 0	62	59	29,91	59		ESE	1	Fair.
18	48	7 0	52	58	29,92	65		S	1	Fine.
	74	2 0	74	61	29,93	56		S	1	Fine.
19	52,5	7 0	54	60	30,07	70		SW	1	Fine.
	71,5	2 0	71	62	30,11	55		WSW	1	Fine.
20	49	7 0	54	61	30,28	67		WSW	1	Hazy.
	69,5	2 0	68,5	63	30,42	56		N	1	Hazy.
21	52	7 0	55	62	30,49	68		SE	1	Fine.
	77	2 0	76	64	30,46	56		SSW	1	Fine.
22	56	7 0	60	64	30,45	62		W	1	Hazy.
	78	2 0	76	68	30,40	56		NW	1	Fine.
23	60	7 0	64	66,5	30,29	64		W	1	Fine.
	81,5	2 0	81	68	30,20	47		NW	1	Fine.
24	50	7 0	53	67	30,16	65		E	1	Cloudy.
	60	2 0	60	67	30,19	56		NE	1	Cloudy.
25	45	7 0	47	65	30,24	59		NE	1	Cloudy.
	56	2 0	55,5	64	30,22	56		E	1	Fine.
26	42	7 0	45	62	30,24	60		NE	1	Cloudy.
	55	2 0	55	61,5	30,24	55		NE	1	Cloudy.
27	36	7 0	43	60	30,37	61		NE	2	Fair.
	53	2 0	53	58	30,37	56		NE	2	Cloudy.
28	38	7 0	43	58	30,42	60		NE	2	Fine.
	56	2 0	56	58	30,30	56		N	2	Fair.
29	46	7 0	48	58	29,85	79	0,145	W	1	Rain.
	61,5	2 0	61	59	29,73	61		NW	2	Fair.
30	45	7 0	46	58	29,90	65		NW	1	Cloudy.
	57	2 0	57	59	29,93	57		NW	1	Cloudy.
31	49	7 0	50	58	29,90	70		SSW	2	Cloudy.
	60	2 0	58	59	29,84	64		S	2	Cloudy.

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1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
June 1	50	7 0	53	58	29,68	66		S	2	Cloudy.
	66	2 0	65	59,5	29,60	58		SSE	2	Cloudy.
2	50	7 0	53	59	29,57	68	0,054	S	2	Cloudy.
	67	2 0	66	61	29,64	57		SSW	2	Fair.
3	52	7 0	52	59	29,84	67		S b. W	2	Cloudy.
	68,5	2 0	68	61	29,82	56		ESE	1	Cloudy.
4	55	7 0	58	60,5	29,72	70	0,046	SE	1	Fine.
	77,5	2 0	76	63,5	29,71	56		SE	1	Fair.
5	59	7 0	62	63	29,78	80	0,473	E	1	Hazy.
	77	2 0	76	65,5	29,75	60		WSW	1	Cloudy.
6	59	7 0	61	64	29,64	79		SW	1	Cloudy.
	76	2 0	70	66	29,60	71		SW	1	Rain.
7	55	7 0	55	65	29,69	73	0,060	N	1	Cloudy.
	59	2 0	59	64	29,75	72		N	1	Cloudy.
8	54	7 0	54	64	29,92	87	0,254	N	1	Rain.
	57	2 0	56	63	29,98	83		NE	1	Rain.
9	51	7 0	53	63	30,05	85	0,505	NE	1	Rain.
	63	2 0	62	63,5	30,10	77		NE	1	Cloudy.
10	50	7 0	52	62,5	30,09	83		ENE	1	Cloudy.
	60	2 0	58	62	30,09	78		NE	1	Cloudy.
11	50	7 0	52	62	30,02	84		NE	1	Cloudy.
	66	2 0	65	63	29,93	68		NE	2	Cloudy.
12	51	7 0	51	62	29,97	73		NE	2	Cloudy.
	56	2 0	56	61,5	30,01	68		NE	2	Cloudy.
13	46	7 0	47	60	30,00	72		NE	1	Cloudy.
	58	2 0	54	60	29,98	71		NE	1	Cloudy.
14	45	7 0	47	59,5	29,91	73		NE	1	Cloudy.
	60	2 0	59	60	29,87	66		NE	2	Cloudy.
15	47	7 0	49	59	29,87	71		NE	1	Cloudy.
	66	2 0	63	60,5	29,87	67		NE	1	Cloudy.
16	52	7 0	53	60	29,99	74		NNW	1	Cloudy.
	60	2 0	59	60	30,03	73		WNW	1	Cloudy.

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		H. M.			Inches.		Inches.	Points.	Str.	
	o		o	o						
June 17	55,5	7 o	57	60	30,05	83		WNW	1	Cloudy.
	59,5	2 o	59	60	30,04	78		E	1	Cloudy.
18	52	7 o	53,5	60	29,96	81	0,043	NE	1	Cloudy.
	54	2 o	53	60	29,90	83		E	1	Rain.
19	41	7 o	41	58	29,95	90	1,490	NE	1	Rain.
	50	2 o	49	57,5	29,94	69		NNE	2	Cloudy.
20	44,5	7 o	46	56	30,02	68	0,032	NE	2	Cloudy.
	52	2 o	51	56	30,03	64		NE	2	Cloudy.
21	42	7 o	44	56	30,14	70		NE	2	Cloudy.
	62	2 o	62	58	30,12	59		NE	1	Fair.
22	49	7 o	52	57	30,08	67		WNW	1	Fair.
	63	2 o	63	58	30,05	58		WNW	1	Cloudy.
23	48	7 o	51	57	29,97	71		SW	1	Cloudy.
	64,5	2 o	61	58	29,98	62		WNW	1	Cloudy.
24	46	7 o	51	58	30,00	70	0,040	W	1	Fair.
	68	2 o	68	59,5	30,00	56		W	1	Cloudy.
25	54	7 o	55	59,5	29,78	73	0,173	WSW	2	Cloudy.
	65	2 o	63	60,5	29,75	73		SW	2	Cloudy.
26	56	7 o	57	59,5	29,50	75	0,065	SW	2	Cloudy.
	69,5	2 o	67	61,5	29,63	63		WNW	1	Cloudy.
27	53	7 o	55	60,5	29,85	75		E	1	Cloudy.
	64	2 o	63	62	29,83	68		E	1	Cloudy.
28	51	7 o	53,5	61	29,71	72	0,062	SSE	2	Cloudy.
	60	2 o	58	61	29,62	70		S	2	Rain.
29	57	7 o	56	61	29,54	72	0,042	S	2	Cloudy.
	68	2 o	67	62	29,57	62		SSW	2	Cloudy.
30	52	7 o	56	61	29,66	68		SW	2	Cloudy.
	62	2 o	56	61	29,54	76		SSE	2	Rain.

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1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
July 1	48	7 0	52	60	29,54	76	0,276	SW	2	Cloudy.
	63	2 0	61	62	29,57	69		SSW	2	Rain.
2	50	7 0	52	61	29,94	71	0,172	NW	1	Cloudy.
	65	2 0	64,5	61	29,98	66		SE	1	Cloudy.
3	53	7 0	54	61	29,93	74		E	1	Cloudy.
	66	2 0	66	62	29,87	66		ESE	1	Cloudy.
4	55	7 0	56	61	29,81	86	0,263	NE	1	Rain.
	60	2 0	59	62	29,96	72		NE	1	Rain.
5	46	7 0	51	61	30,20	66	0,143	NW	1	Fine.
	67	2 0	66	62	30,16	66		N	1	Fair.
6	48	7 0	52	61	30,22	70		N	1	Fine.
	63	2 0	62	62	30,24	61		NE	1	Cloudy.
7	50	7 0	51	61,5	30,26	74		E	1	Fair.
	65	2 0	63	62	30,26	64		NE	1	Fair.
8	51	7 0	51,5	62	30,26	72		NE	1	Cloudy.
	62	2 0	62	62	30,26	66		NE	1	Cloudy.
9	50,5	7 0	52	62	30,23	69		NE	1	Fair.
	64	2 0	63	62,5	30,17	61		NE	1	Fair.
10	51	7 0	52	61	30,13	75		NE	1	Cloudy.
	60	2 0	60	62,5	30,12	65		NE	1	Cloudy.
11	50	7 0	51	61	30,08	71		NE	1	Cloudy.
	61	2 0	58,5	61,5	30,05	66		NE	1	Cloudy.
12	50	7 0	51	60	30,00	72		NE	1	Cloudy.
	58	2 0	56,5	60,5	29,99	69		NE	1	Cloudy.
13	51,5	7 0	53	60	29,94	68		NE	2	Cloudy.
	56	2 0	56	60	29,96	66		NE	2	Cloudy.
14	49	7 0	52	59	30,12	68		NE	2	Cloudy.
	66	2 0	66	61	30,12	59		NE	1	Fine.
15	57	7 0	57	61	30,14	70		NW	1	Fair.
	64	2 0	58	61	30,11	75		NE	1	Rain.
16	47	7 0	53	60	30,12	71	0,033	NW	1	Cloudy.
	69,5	2 0	68,5	62	30,06	60		WNW	1	Fair.

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		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
July 17	58	7 0	60	61	29,87	71		WNW	1	Cloudy.
	65	2 0	63,5	62	29,87	65		NW	2	Cloudy.
18	52	7 0	53	61	30,00	65		WNW	1	Cloudy.
	66,5	2 0	65	62	29,98	63		WSW	1	Cloudy.
19	58	7 0	58	62	30,00	69		WSW	1	Cloudy.
	73	2 0	71,5	64,5	30,00	58		SW	1	Fair.
20	55	7 0	56,5	63	29,98	70		SW	1	Fine.
	76	2 0	74	67	29,94	60		S	2	Fine.
21	59	7 0	62	64,5	29,81	69		NE	1	Fair.
	76	2 0	75	68	29,72	63		E	1	Fair.
22	58	7 0	60	66	29,67	72	0,050	SW	2	Cloudy.
	69	2 0	67	66	29,69	66		SW	2	Cloudy.
23	53	7 0	54	64	29,78	66		W	2	Cloudy.
	66	2 0	65	64,5	29,76	64		W	2	Fair.
24	51	7 0	54	64	29,76	71		SW	1	Cloudy.
	65,5	2 0	61	64	29,64	64		SW	2	Cloudy.
25	51	7 0	53	64	29,76	70	0,055	S	1	Fair.
	63	2 0	61	64	29,78	70		ESE	1	Fair.
26	52	7 0	53,5	63	30,04	71	0,240	NE	1	Fair.
	69,5	2 0	68	64,5	30,05	59		NW	1	Fine.
27	53	7 0	56	64	30,05	67		SW	1	Hazy.
	66	2 0	63	64	30,05	70		SW	1	Cloudy.
28	61	7 0	62	64	29,94	78	0,060	WSW	1	Cloudy.
	72,5	2 0	70	66	29,98	67		WSW	1	Cloudy.
29	61	7 0	63	65,5	30,04	71		SW	1	Cloudy.
	70	2 0	70	67	29,97	64		SW	2	Cloudy.
30	58	7 0	60	65,5	29,89	76	0,077	SW	1	Cloudy.
	72,5	2 0	72	67	29,89	59		W	1	Fair.
31	55	7 0	58	66	29,84	71	0,031	SW	1	Fine.
	70	2 0	69	66,5	29,82	64		WSW	1	Fair.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Aug. 1	54	7 0	57	65	29,78	75	0,038	S	2	Rain.
	64	2 0	63	65	29,71	72		S	2	Cloudy.
2	57	7 0	59	65	29,64	75	0,225	SSW	2	Cloudy.
	67,5	2 0	67	65	29,65	64		SSW	2	Cloudy.
3	56	7 0	58	64,5	29,88	71	0,163	WSW	2	Fair.
	70,5	2 0	70	66	29,97	59		WSW	2	Fair.
4	57	7 0	58	65,5	30,01	78	0,170	S	1	Rain.
	65	2 0	61	65,5	29,96	75		E	1	Cloudy.
5	52	7 0	54	64,5	30,13	77		SW	1	Fair.
	70	2 0	68	67	30,13	64	0,270	E	1	Fair.
6	57	7 0	59	65	30,12	77		E	1	Cloudy.
	70	2 0	69	68	30,12	62		E	1	Fine.
7	54	7 0	58	65	30,02	73		E	1	Cloudy.
	66	2 0	65	65,5	29,96	70		S	2	Cloudy.
8	56	7 0	58	65	29,96	73		S	2	Fair.
	71	2 0	70	66	29,95	58		S	1	Cloudy.
9	56	7 0	58	65,5	29,97	72	0,053	S	1	Fair.
	71	2 0	70	66	29,97	62		SW	1	Fair.
10	59,5	7 0	61	66	29,99	74		WSW	1	Fair.
	73	2 0	71	68	29,98	63		WSW	1	Fair.
11	64	7 0	64,5	67	30,01	75		WSW	1	Cloudy.
	75	2 0	74,5	68,5	30,04	66		SW	1	Cloudy.
12	66	7 0	68	68	30,01	75		WSW	1	Cloudy.
	79	2 0	78	70	29,96	64		SE	1	Cloudy.
13	62	7 0	64	69	29,86	78		E	1	Hazy.
	79	2 0	78	73	29,78	64		E	1	Fine.
14	64	7 0	65	70	29,75	81	0,265	WSW	1	Cloudy.*
	73	2 0	72,5	71,5	29,80	69		WNW	1	Cloudy.
15	61	7 0	63	70	29,82	76		NW	1	Cloudy.
	72,5	2 0	69	69,5	29,82	70		NW	1	Cloudy.
16	53	7 0	55	69	29,77	75		SSW	1	Hazy.
	71	2 0	71	69	29,79	65		SSW	1	Fine.

* much thunder & lightning last night.

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		H. M.			Inches.		Inches.	Points.	Str.	
	o		o	o						
Aug. 17	56	7 o	58	68	29,72	73		SSW	2	Cloudy.
	68,5	2 o	64,5	68	29,62	75		S	2	Cloudy.
18	52	7 o	55	67	29,79	73	0,167	W	2	Fine.
	69	2 o	68	68	29,81	57		WNW	2	Fair.
19	51	7 o	54	66	30,02	74		SW	1	Fine.
	70,5	2 o	68	67	30,01	67		S	2	Cloudy.
20	63	7 o	64	67	30,05	74		S	2	Fine.
	77	2 o	76	69,5	30,07	65		SSW	1	Fair.
21	61	7 o	63	68	29,94	77		NE	1	Cloudy.
	79	2 o	78	70	29,80	75		SE	1	Cloudy.
22	54,5	7 o	56	68	29,73	76	0,487	SW	1	Fine.
	68	2 o	65	68	29,73	64		SW	1	Fair.
23	51	7 o	53	68,5	29,92	75	0,018	SW	1	Fine.
	72	2 o	71	68	29,97	62		SW	1	Fine.
24	57	7 o	62	68	30,02	73		SSW	1	Cloudy.
	70	2 o	70	68	30,08	67		SSW	1	Cloudy.
25	59	7 o	63	67	30,18	64		S	1	Cloudy.
	73,5	2 o	73	69	30,17	62		E	1	Fair.
26	57,5	7 o	60	68,5	30,09	71		E	1	Fine.
	75	2 o	74,5	70	30,00	63		SE	1	Fine.
27	58	7 o	59	68	29,98	71		WNW	1	Fine.
	70	2 o	68	70	30,04	60		NW	1	Fine.
28	52	7 o	54	66	30,20	69		WNW	1	Fair.
	67	2 o	66	68	30,23	60		WNW	1	Fine.
29	52	7 o	55	67	30,31	67		W	1	Cloudy.
	70	2 o	68	68	30,29	59		SE	1	Fine.
30	51,5	7 o	54	65	30,26	71		SE	1	Fine.
	70	2 o	70	68	30,25	61		SSE	1	Fine.
31	51,5	7 o	54	64	30,25	70		ENE	1	Fine.
	70	2 o	70	67,5	30,25	61		E	1	Fine.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Sept. 1	53	7 0	55	63,5	30,14	79		NE	1	Cloudy.
	73	2 0	73	69	30,07	66		ENE	1	Fine.
2	58,5	7 0	61	66	29,93	81		NE	1	Fine.
	71,5	2 0	71,5	70,5	29,89	69		ENE	1	Fine.
3	58,5	7 0	63,5	67	29,81	76		ENE	1	Fine.
	75	2 0	74,5	71,5	29,80	65		E	2	Fine.
4	64	7 0	65	69	29,64	69		E	2	Cloudy.
	68	2 0	67	69	29,64	70		S	2	Cloudy.
5	60,5	7 0	63	67,5	29,74	76	0,061	S	2	Cloudy.
	73	2 0	72	69,5	29,85	66		S	2	Cloudy.
6	63	7 0	65	69	29,89	71		S b. W	1	Fine.
	77	2 0	76,5	72	29,90	62		S b. W	1	Fine.
7	62	7 0	63,5	70	30,04	74		S b. W	1	Fine.
	78	2 0	77,5	73	30,04	66		E	1	Fine.
8	67	7 0	67,5	71	30,01	75		ESE	1	Cloudy.
	72	2 0	71	71	30,06	70		SSW	2	Cloudy.
9	61	7 0	63	70	30,14	73		SSW	2	Cloudy.
	72	2 0	71	71	30,13	65		W	2	Fine.
10	60	7 0	61,5	69	30,20	75		W	1	Cloudy.
	74	2 0	72,5	70,5	30,20	65		W	1	Hazy.
11	61,5	7 0	62	70	30,20	74		S	1	Cloudy.
	75	2 0	74	71	30,24	64		S	1	Fine.
12	55	7 0	59	68	30,36	68		NE	1	Fine.
	70,5	2 0	70	71	30,35	64		E	1	Hazy.
13	58	7 0	60	68	30,33	74		NE	1	Cloudy.
	70,5	2 0	69,5	70	30,32	64		NE	1	Hazy.
14	55	7 0	58	66	30,37	70		W	1	Cloudy.
	70	2 0	69	69	30,39	65		NE	1	Cloudy.
15	49	7 0	52	64	30,46	71		NE	1	Fine.
	65	2 0	65	68	30,43	64		E	1	Fine.
16	49,5	7 0	52	66	30,35	74		NE	1	Fine.
	65	2 0	63,5	68	30,29	66		E	1	Fine.

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		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Sept. 17	52	7 0	54	64,5	30,17	77		E	1	Fine.
	69	2 0	69	68	30,10	62		E	1	Fine.
18	52	7 0	53	65,5	29,98	76		ENE	1	Fair.
	73	2 0	73	70	29,96	65		S	1	Fine.
19	58	7 0	61	67	29,93	73		E	1	Fair.
	77	2 0	77	71	29,93	64		ESE	1	Fine.
20	58	7 0	60	68	29,93	74		E	1	Fine.
	75,5	2 0	75	71	29,95	65		E	1	Fine.
21	54	7 0	56,5	67	30,09	78		NE	1	Fine.
	67,5	2 0	67	70	30,11	63		E	1	Fine.
22	50,5	7 0	52	65	30,11	72		NNE	1	Fine.
	68,5	2 0	68,5	68,5	30,09	62		ESE	1	Fine.
23	48	7 0	50	64,5	30,09	70		S	1	Fine.
	70	2 0	70	68	30,10	61		NW	1	Cloudy.
24	50	7 0	53	64	30,23	68		NE	1	Hazy.
	64,5	2 0	64	67	30,22	62		W	1	Hazy.
25	53,5	7 0	56	64	30,18	73		WNW	1	Cloudy.
	72	2 0	71	68,5	30,16	65		WNW	1	Fine.
26	58	7 0	59	66	30,05	72		SSW	1	Cloudy.
	69	2 0	65	67	30,07	73		NW	1	Cloudy.
27	45	7 0	46	64,5	30,34	72	0,020	NE	1	Fair.
	60,5	2 0	60	66	30,31	61		E	1	Fine.
28	46	7 0	48	64	30,19	68		ENE	2	Fine.
	60	2 0	60	65,5	30,06	62		E	2	Fine.
29	52	7 0	53	64	29,84	69		E	2	Cloudy.
	68	2 0	67	64,5	29,79	69		SE	1	Cloudy.
30	58	7 0	60	64	29,91	84		S	1	Cloudy.
	69,5	2 0	68	66,5	29,98	67		S	2	Fair.

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1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Oct. 1	52	7 0	53	64	30,13	80		NE	1	Cloudy.
	68	2 0	67	66	30,05	74		E	1	Cloudy.
2	59	7 0	59	65	30,00	84	0,071	S	2	Cloudy.
	67	2 0	66	66	30,04	69		SSW	2	Fair.
3	54	7 0	53	65,5	30,09	80		NE	1	Foggy.
	61	2 0	61	65	29,97	75		NNE	1	Cloudy.
4	56	7 0	57	64,5	29,77	87	0,168	NE	1	Cloudy.
	59	2 0	59	65	29,77	85		NE	1	Rain.
5	52	7 0	52,5	63,5	29,84	84	0,286	SW	1	Fair.
	64	2 0	62	64,5	29,84	71		SSW	1	Cloudy.
6	48	7 0	48	63	29,97	79		WSW	1	Fair.
	58	2 0	56,5	63	30,07	65		WNW	1	Fair.
7	42	7 0	44	62	30,18	74		WSW	1	Cloudy.
	59	2 0	59	63	30,07	64		E	1	Fine.
8	50	7 0	52	62	29,72	78		E	1	Cloudy.
	54,5	2 0	54	62	29,55	83		E	1	Rain.
9	50	7 0	52	61	29,36	83	0,348	S	1	Fair.
	58	2 0	56,5	61	29,16	83		E	2	Rain
10	45,5	7 0	47	60	29,24	77	0,260	SE b. S	2	Fair.
	58	2 0	58	61	29,19	74		S	2	Hazy.
11	47	7 0	48	59	29,16	80	0,210	E	1	Cloudy.
	58,5	2 0	58	60	29,18	69		S	2	Rain.
12	46,5	7 0	47	59	29,50	83	0,128	W	1	Fine.
	58,5	2 0	58	60	29,62	68		W	1	Hazy.
13	53	7 0	55	59	29,68	86	0,230	E	1	Cloudy.
	61	2 0	61	61	29,72	84		S	1	Cloudy.
14	56	7 0	58	61	29,76	85	0,160	SE	1	Cloudy.
	66,5	2 0	65	63	29,68	76		SE	2	Cloudy.
15	57	7 0	57	62	29,51	80	0,118	S	2	Cloudy.*
	63,5	2 0	62,5	64,5	29,70	69		SW	2	Fair.
16	55	7 0	58	62,5	29,78	76		SSW	2	Cloudy.
	61	2 0	60	64	29,71	76		SSW	2	Cloudy.

* much lightning with thunder last night.

much wind last night.

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for October, 1795.

1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°						
Oct. 17	52	7 0	53	62	29,90	79	0,066	SW	1	Fine.
	61	2 0	60,5	64	29,97	70		SSW	1	Hazy.
18	53	7 0	56	63	29,74	84		ESE	1	Cloudy.
	62	2 0	62	64	29,68	76		SSW	2	Cloudy.
19	54	7 0	55	63	29,73	86		E	1	Cloudy.
	66,5	2 0	66	66	29,67	73		S	1	Fair.
20	59,5	7 0	60	64	29,66	82		SE	1	Fair.
	65	2 0	65	66	29,60	74		S	2	Cloudy.
21	55	7 0	55	64	29,54	78	0,028	S	2	Fair.
	63	2 0	61	64	29,43	76		SSW	2	Cloudy.
22	52	7 0	52	63	29,26	86	0,102	E	1	Rain.
	58	2 0	58	65	29,18	73		SSW	2	Fair.
23	44	7 0	45	62	29,23	79	0,156	SW	2	Cloudy.
	58,5	2 0	55	64,5	29,35	65		SW	2	Fair.
24	51	7 0	57	62	29,14	70		SSW	2	Cloudy.
	62	2 0	61,5	64	29,33	65		SW	2	Fair.
25	51,5	7 0	52	62	29,72	72		SW	2	Fine.
	60,5	2 0	59,5	64	29,84	63		W	2	Fair.
26	49	7 0	49	62	29,95	79		W	1	Fair.
	57	2 0	57	64	29,95	68		W	1	Fair.
27	44,5	7 0	47	61	29,82	78		SW	1	Fair.
	58	2 0	58	61,5	29,72	71		SSW	2	Cloudy.
28	54	7 0	55	61	29,50	81		S	2	Cloudy. [much wind last night.
	60	2 0	59	63,5	29,43	69		S	2	Fair.
29	48	7 0	48	60	29,52	71		SSW	2	Fair.
	54	2 0	54	62	29,62	64		SSW	2	Fine.
30	46	7 0	48	59	29,50	76		SSW	2	Cloudy.
	51,5	2 0	51,5	61	29,65	68		SW	2	Cloudy.
31	44	7 0	49	59	29,87	79	0,208	SSW	2	Cloudy.
	58	2 0	56,5	61	29,74	70		S	2	Cloudy.

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for November, 1795.

1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Nov. 1	52	7 0	53	59,5	29,44	81	0,476	SE	2	Cloudy.
	55	2 0	53,5	61	29,27	76		SSE	2	Fair.
2	44	7 0	45	58,5	29,31	77	0,250	S	1	Cloudy.
	49	2 0	49	60	29,46	73		NE	1	Cloudy.
3	38	7 0	39	56	30,02	74		NE	1	Fair.
	45	2 0	44,5	58,5	30,09	67		NE	1	Cloudy.
4	33	7 0	34	55	30,22	74		NW	1	Fine.
	44	2 0	42,5	57	30,16	69		SW	1	Fine.
5	41	7 0	50	56	29,79	86	0,118	SW	2	Cloudy.
	53	2 0	51,5	58	29,88	75		WNW	1	Cloudy.
6	46	7 0	46	56	29,84	73		NW	1	Cloudy. [much wind last night.
	51	2 0	50,5	58	30,15	67		NW	1	Fine.
7	42	7 0	47	56	30,25	77		SW	1	Cloudy.
	56	2 0	56	58,5	30,15	72		NW	1	Fine.
8	44	7 0	46	57	30,28	74		N	1	Fair.
	52	2 0	51	58,5	30,31	69		NE	1	Cloudy.
9	41	7 0	41	56,5	30,40	82		NE	1	Cloudy.
	49	2 0	47	58	30,45	64		E	1	Fine.
10	39	7 0	40	56	30,48	79		ENE	1	Fine.
	49	2 0	47	57	30,50	67		E	1	Fine.
11	39	7 0	40	56	30,58	77		ENE	1	Fair.
	49	2 0	49	56	30,56	70		NE	1	Cloudy.
12	41	7 0	41	56	30,48	76		NE	1	Cloudy.
	49	2 0	48	56,5	30,34	74		WNW	1	Cloudy.
13	49	7 0	49	57	30,11	78		WNW	1	Cloudy.
	53	2 0	53	58	30,02	73		NW	1	Cloudy.
14	36	7 0	36	57	30,05	74		N	2	Fine.
	43	2 0	42	56,5	30,12	71		N	2	Fair.
15	34	7 0	34	55	30,35	75		NE	1	Fair.
	42	2 0	41,5	58	30,40	71		NE	1	Fine.
16	31	7 0	33	54	30,40	75		WSW	1	Cloudy.
	42,5	2 0	42,5	55	30,30	74		NW	1	Cloudy.

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for November, 1795.

1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	o		o	o						
Nov. 17	40	7 0	40	54	30,14	80		WSW	1	Cloudy.
	48	2 0	47	56	30,03	73		SW	2	Cloudy.
18	45	7 0	46	54	29,55	78		S	3	Cloudy.
	50	2 0	49	55	29,15	80		S	3	Rain.
19	38	7 0	39	54	29,27	79	0,702	SW	1	Cloudy.
	45	2 0	45	55	29,27	73		SSW	1	Fair.
20	34,5	7 0	35	54	29,29	77		SW	1	Fair.
	40	2 0	40	55	29,48	75		NE	1	Cloudy.
21	26	7 0	27	52	29,75	74		WSW	1	Fine.
	39	2 0	38	54	29,80	70		SW	1	Fine.
22	36	7 0	45	53	29,55	85	0,038	S	2	Cloudy.
	51	2 0	50	55	29,44	81		S	2	Cloudy.
23	36	7 0	36	53	29,78	80		SW	1	Fair.
	44	2 0	44	55	29,88	74		WSW	1	Fine.
24	39	7 0	48	53	29,28	85	0,167	S	2	Rain.
	51	2 0	50	56	29,17	81		S	2	Cloudy.
25	41	7 0	42	54	29,00	82		S	1	Cloudy.
	46,5	2 0	43	55	28,94	80		NE	2	Cloudy.
26	28,5	7 0	29	51	29,40	76		W	1	Fine.
	37	2 0	37	53	29,44	71		NW	1	Fine.
27	30	7 0	30	51	29,77	77		WNW	1	Fair.
	39	2 0	38	54	29,74	74		SSE	1	Cloudy.
28	36	7 0	36	51	29,30	87	0,377	NE	1	Cloudy.
	40	2 0	40	52	29,62	80		NE	1	Fair.
29	28	7 0	30	49	30,15	78		WSW	1	Fair.
	40	2 0	40	51	30,14	76		W	1	Cloudy.
30	39	7 0	47	51	29,95	90	0,300	SW	1	Cloudy.
	54	2 0	53	55	29,92	89		SW	1	Cloudy.

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for December, 1795.

1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	°	H. M.	°	°	Inches.		Inches.	Points.	Str.	
Dec. 1	48	8 0	48	54	30,09	88		WSW	1	Cloudy.
	54	2 0	54	57	30,07	86		SW	1	Cloudy.
2	50	8 0	50	56	29,93	85	0,025	WNW	1	Rain.
	51	2 0	49	57	29,92	71		WNW	1	Fair.
3	43	8 0	44	56	29,90	75		WNW	1	Cloudy.
	48	2 0	48	57	29,90	73		NW	1	Fair.
4	41	8 0	42	55	30,08	76		NW	1	Cloudy.
	47	2 0	46,5	56,5	30,11	73		NW	1	Cloudy.
5	40	8 0	41	56	30,14	81		SW	1	Cloudy.
	49	2 0	49	57	30,03	85		SW	1	Cloudy.
6	47	8 0	51	56	30,03	88		WSW	1	Cloudy.
	54	2 0	54	58,5	30,08	84		NW	1	Cloudy.
7	47	8 0	47	57	30,13	86		WSW	1	Cloudy.
	49	2 0	49	58	30,16	81		WSW	1	Cloudy.
8	45	8 0	46	57	30,21	86		NNE	1	Cloudy.
	47	2 0	46	58	30,25	86		NE	1	Cloudy.
9	34	8 0	36	56	30,38	80		E	1	Fine.
	42	2 0	40	57	30,39	80		NE	1	Cloudy.
10	39	8 0	39	55	30,35	80		E	1	Cloudy.
	42	2 0	41	56	30,31	80		E	1	Cloudy.
11	40	8 0	41	56	30,23	80		E	1	Cloudy.
	43	2 0	42	56	30,19	80		E	1	Cloudy.
12	41	8 0	41	54	29,97	83		E	2	Cloudy.
	44	2 0	44	57	29,83	76		ESE	2	Cloudy.
13	41	8 0	45	55	29,53	83	0,157	SE	2	Cloudy.
	52	2 0	51	57	29,42	80		SSE	2	Cloudy.
14	47	8 0	48	56	29,56	84	0,060	ESE	1	Cloudy.
	52	2 0	51	57	29,49	84		SSE	1	Cloudy.
15	44	8 0	46	56	29,78	84		SSE	1	Fair.
	51,5	2 0	50,5	59	29,78	80		SSE	1	Fair.
16	45	8 0	46	57	29,70	84		ESE	1	Fair.
	52	2 0	52	59	29,64	82		SE	1	Cloudy.

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for December, 1795.

1795	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°									
Dec. 17	47	8 0	50	57	29,58	80		SE	2	Cloudy.
	54	2 0	52,5	59	29,62	77		SSE	2	Cloudy.
18	47	8 0	48	58	29,71	80		SE	1	Fair.
	53	2 0	52	61	29,71	79		SE	1	Cloudy.
19	49	8 0	50	59,5	29,60	84		S	2	Cloudy.
	54	2 0	54	62	29,51	79		S	2	Fair.
20	48	8 0	48	59	29,50	83		S	1	Cloudy.
	52	2 0	52	62	29,56	77		SW	1	Fair.
21	44,5	8 0	45	58	29,92	81	0,056	SE	1	Rain.
	53	2 0	52	60	29,84	85		S	2	Rain.
22	47	8 0	54	59	29,71	86	0,132	SSW	2	Cloudy.
	56	2 0	54	61	29,71	78		SSW	2	Cloudy.
23	43	8 0	43	57	29,93	78		SW	2	Fine.
	48	2 0	47	59	30,11	72		SW	1	Fine.
24	47	8 0	48	57	30,25	81		SW	1	Cloudy.
	54	2 0	53	60	30,16	84		SW	2	Cloudy.
25	43	8 0	43	60	30,31	75	0,097	SW	1	Fair.
	46	2 0	46	62	30,34	73		WNW	1	Cloudy.
26	38	8 0	38	57	30,22	77		NNE	1	Fine.
	42	2 0	42	60	30,32	74		NE	1	Fine.
27	34	8 0	37	55	30,32	78		SW	1	Cloudy.
	43	2 0	43	59,5	30,22	75		S	2	Fair.
28	38,5	8 0	48	57	29,94	82		S	2	Cloudy.
	50	2 0	47	59	30,06	77		SSW	2	Fair.
29	44	8 0	52	57	29,78	86	0,048	SSW	2	Cloudy.
	55	2 0	55	59,5	29,75	84		SSW	2	Cloudy.
30	39	8 0	40	54	30,13	80	0,398	W	2	Fair.
	43	2 0	43	58	30,32	74		NW	2	Fine.
31	35	8 0	44	55	30,14	80		SW	2	Cloudy.
	44	2 0	44	57,5	29,98	80		SW	2	Cloudy.

1795.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	46,0	7	26,0	46	8	26,0	49	36	43,4	30,47	29,16	30,01	92	66	71,2	0,476
February	51	24	35,9	51	25	36,4	55	42	47,9	30,68	29,04	29,60	91	64	77	1,255
March	54,5	24	40,4	54,5	25	41,1	59	47	52,7	30,35	29,02	29,80	85	56	72,6	1,744
April	59,5	36	47,9	58,5	37	48	61,5	53	57,5	30,22	29,34	29,79	80	54	69,7	0,497
May	81,5	36	54,5	81	43	55,8	68	57	61,1	30,49	29,73	30,17	79	47	61,5	0,276
June	77,5	41,0	56,6	76	41	57,2	66	56	61,1	30,14	29,50	29,86	90	56	71,0	3,339
July	76	46	64,0	75	51	59,8	68	59	62,8	30,26	29,54	29,97	86	58	68,0	1,400
August	79	51	63,9	78	53	64,3	73	64	67,4	30,31	29,62	29,97	81	57	69,2	1,856
September	78	45	63,1	77,5	46	63,7	73	63,5	67,8	30,46	29,64	30,08	84	61	69,3	0,081
October	68	42	55,6	67	44	55,8	66	59	62,6	30,18	29,16	29,66	87	64	75,7	2,539
November	56	28	42,4	56	27	43,1	61	49	55,4	30,58	28,94	29,87	90	64	76,1	2,428
December	56	34	46,1	55	36	46,9	62	54	57,5	30,39	29,42	29,97	88	71	80,4	0,973
Whole year			49,7			49,9			58,1			29,90			71,8	16,864

* The quicksilver in the bason of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCVI.

PART II.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCVI.

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PHILOSOPHICAL TRANSACTIONS.

XI. *Of the Influence of Cold upon the Health of the Inhabitants of London.* *By* William Heberden *Jun. M. D. F. R. S.*

Read March 10, 1796.

THE extraordinary mildness of last January, compared with the unusual severity of the January preceding, affords a peculiarly favourable opportunity of observing the effect of each of these seasons contrasted with each other. For of these two successive winters, one has been the coldest, and the other the warmest, of which any regular account has ever been kept in this country. Nor is this by any means an idle speculation, or matter of mere curiosity; for one of the first steps towards preserving the health of our fellow-creatures, is to point out the sources from which diseases are to be apprehended. And what may make the present inquiry more particularly useful, is that the result, as I hope clearly to make appear by the following statements, is entirely contrary to the prejudices usually entertained upon this subject.

During last January, nothing was more common than to hear expressions of the unseasonableness of the weather; and fears lest the want of the usual degree of cold, should be productive of putrid diseases, and I know not what other causes of mortality. On the other hand, "a bracing cold," and "a clear frost," are familiar in the mouth of every Englishman; and what he is taught to wish for, as among the greatest promoters of health and vigour.

Whatever deference be due to received opinions, it appears to me however from the strongest evidence, that the prejudices of the world are upon this point at least unfounded. The average degrees of heat upon FAHRENHEIT's thermometer kept in London during the month of January 1795, was 23° in the morning, and 29°.4 in the afternoon. The average in January 1796, was 43°.5 in the morning, and 50°.1 in the afternoon. A difference of above twenty degrees! And if we turn our attention from the comparative coldness of these months, to the corresponding healthiness of each, collected from the weekly bills of mortality, we shall find the result no less remarkable. For in five weeks between the 31st of December 1794 and the 3d of February 1795, the whole number of burials amounted to 2823; and in an equal period of five weeks between the 30th of December 1795 and the 2d of February 1796, to 1471. So that the excess of the mortality in January 1795 above that of January 1796, was not less than of 1352 persons. A number sufficient surely to awaken the attention of the most prejudiced admirers of a frosty winter. And though I have only stated the evidence of two years, the same conclusion may universally be drawn; as I have learned from a careful examination of the weekly bills of mortality for

many years. These two seasons were chosen as being each of them very remarkable, and in immediate succession one to the other, and in every body's recollection.

It may not be impertinent to the objects of this Society, without entering too much into the province of medicine, to consider a little more particularly the several ways in which this effect may be supposed to be produced; and to point out some of the principal injuries which people are liable to sustain in their health from a severe frost. And one of the first things that must strike every mind engaged in this investigation, is its effect on old people. It is curious to observe among those who are said in the bills to die above 60 years of age, how regularly the tide of mortality follows the influence of this prevailing cause: so that a person used to such inquiries, may form no contemptible judgment of the severity of any of our winter months, merely by attending to this circumstance. Thus their number last January was not much above $\frac{1}{5}$th of what it had been in the same month the year before. The article of asthma, as might be expected, is prodigiously increased, and perhaps includes no inconsiderable part of the mortality of the aged. After these come apoplexies and palsies, fevers, consumptions, and dropsies. Under the two last of which are contained a large proportion of the chronical diseases of this country; all which seem to be hurried on to a premature termination. The whole will most readily be seen at one view in the following table.

1795.

Week ending	Mean heat.		Whole No. of deaths.	Aged above 60.	Asthma.	Apoplexy and palsy.	Fever.	Consumption.	Dropsy.
	Morn.	Noon.							
6 Jan.	25°	29°	244	51	13	4	20	73	7
13 Jan.	26°	32°	532	139	26	13	49	158	20
20 Jan.	24°	30°	637	145	51	11	81	164	37
27 Jan.	19°	27°	543	143	64	11	42	157	17
3 Feb.	25°	37°	867	239	95	13	66	273	45
Result	23°	29°.4	2823	717	249	52	258	825	126

1796.

Week ending	Mean heat.		Whole No. of deaths.	Aged above 60.	Asthma.	Apoplexy and palsy.	Fever.	Consumption.	Dropsy.
	Morn.	Noon.							
5 Jan.	40°	46°	300	35	5	7	34	79	13
12 Jan.	41°	49°	273	37	9	5	25	53	19
19 Jan.	48°	53°	313	29	2	4	29	77	11
26 Jan.	47°	52°	257	20	7	9	23	47	11
2 Feb.	41°	49°	328	32	6	6	23	86	16
Result	43°.5	50°.1	1471	153	29	31	134	342	70

Notwithstanding the plague, the remittent fever, the dysentery, and the scurvy, have so decreased, that their very name is almost unknown in London; yet there has, I know not how, arisen a prejudice concerning putrid diseases, which seems to have made people more and more apprehensive of them, as the danger has been growing less. It must in great measure be attributed to this, that the consumption of Peruvian bark

in this country has, within the last fifty years, increased from 14,000 to above 100,000 pounds annually. And the same cause has probably contributed, from a mistaken mode of reasoning, to prepossess people with the idea of the wholesomeness of a hard frost. But it has in another place* been very ably demonstrated that a long frost is eventually productive of the worst putrid fevers that are at this time known in London; and that heat does in fact prove a real preventive against that disease. And although this may be said to be a very remote effect of the cold, it is not therefore the less real in its influence upon the mortality of London. Accordingly a comparison of the numbers in the foregoing table will shew that very nearly twice as many persons died of fevers in January 1795, as did in the corresponding month of this year. I might go on to observe that the true scurvy was last year generated in the metropolis from the same causes extended to an unusual length. But these are by no means the only ways, nor indeed do they seem to be the principal ways, in which a frost operates to the destruction of great numbers of people. The poor, as they are worse protected from the weather, so are they of course the greatest sufferers by its inclemency. But every physician in London, and every apothecary, can add his testimony, that their business among all ranks of people never fails to increase, and to decrease, with the frost. For if there be any whose lungs are tender, any whose constitution has been impaired either by age, or by intemperance, or by disease; he will be very liable to have all his complaints increased, and all his infirmities aggravated by such a season. Nor must the young and active think themselves quite secure,

* Observations on the Jail Fever, by Dr. HUNTER, Med. Trans. Vol. III.

or fancy their health will be confirmed by imprudently exposing themselves. The stoutest man may meet with impediments to his recovery from accidents otherwise inconsiderable; or may contract inflammations, or coughs, and lay the foundation of the severest ills. In a country where the prevailing complaints among all orders of people are colds, coughs, consumptions, and rheumatisms, no prudent man can surely suppose that unnecessary exposure to an inclement sky; that priding oneself upon going without any additional clothing in the severest winter; that inuring oneself to be hardy, at a time that demands our cherishing the firmest constitution lest it suffer; that braving the winds, and challenging the rudest efforts of the season, can ever be generally useful to Englishmen. But if generally, and upon the whole, it be inexpedient, then ought every one for himself to take care that he be not the sufferer. For many doctrines very importantly erroneous; many remedies either vain, or even noxious, are daily imposed upon the world for want of attention to this great truth; that it is from general effects only, and those founded upon extensive experience, that any maxim to which each individual may with confidence defer, can possibly be established.

XII. *An Analysis of the* Carinthian *Molybdate of Lead; with Experiments on the Molybdic Acid. To which are added some Experiments and Observations on the Decomposition of the Sulphate of Ammoniac.* *By* Charles Hatchett, *Esq. Communicated by Sir* Joseph Banks, *Bart. K.B. P.R.S.*

Read April 14, 1796.

§ I.

THE celebrated SCHEELE, in 1778, read before the Academy of Sciences at Stockholm an essay, in which he proved, by a series of ingenious experiments, that the mineral called *Molybdæna* was composed of sulphur, and a peculiar metallic substance, which, like arsenic and tungsten, was liable by superoxygenation to be converted into a metallic acid, which in its properties differed from any other that had been previously discovered.*

The experiments of M. PELLETIER,† Mr. ISLMANN,‡ and Mr. HIELM,§ confirmed the discovery of the Swedish chemist; but the existence of this metallic substance was only known to be in that mineral which SCHEELE had examined, as no vestige of it had as yet been discovered in other bodies ap-

* SCHEELE's Essays, translated by Dr. BEDDOES, p. 227.

† *Journ. de Physique, Decembre,* 1785.

‡ *Chemische Annalen von* CRELL, 1787, *et Journ. de Physique, Octobre,* 1788.

§ *Journ. de Physique, Mai,* 1789.

pertaining to the mineral kingdom. Mr. JACQUIN, in 1781 and 1786, gave to the public an account, from the Abbé WULFEN, of a yellow sparry lead ore, found at Villach in Carinthia;* and the Abbé WULFEN himself, in an elaborate work, written in the German language, and published in 1785, also described the abovementioned lead ore, together with some experiments which had been made on it.†

Nothing satisfactory, however, relative to its nature was exposed in these memoirs; and although the substance was indisputably proved to be an ore of lead, yet the mineralizing principle of it remained unknown.

In 1790, Mr. HEYER, of Brunswick, made some experiments on this ore, from which he inferred that it was composed of lead, combined with the tungstic acid;‡ and in the same year Mr. KLAPROTH communicated to Dr. CRELL a similar account, which he had received from Mr. SALZWEDEL, of Frankfort sur Mayne.§

This substance was therefore universally believed to be a tungstate of lead, till that excellent chemist Mr. KLAPROTH undertook to subject it to a farther examination; and as the experiments which I have made may be regarded as a continuation of those made by Mr. KLAPROTH, I think it necessary here to mention them.

* NICOLAI JOSEPHI JACQUIN *Miscellanea Austriaca*, Tom. II. p. 139; *et* N. J. JACQUIN *Collectanea*, Tom. I. p. 3.

This ore is also described in *Lithophyl. Bornianum*, Tom. I. p. 90; *et* Tom. II. p. 123.—KARSTEN *in Mus. Lesk.* Tom. II. p. 501.—WERNER's *Verzeich.* Band, I. p. 128.—RAAB's *Catal.* Tome II. p. 379.—*Romé de l'Isle Cristal.* Tome III. p. 387.—WIDENMANN's *Handbuch der Mineralogie*, p. 864.

† XAVIER WULFENS *Abhandlung vom Kärnthnerischen Bleyspate. Wien*, 1785.

‡ *Chemische Annalen von* CRELL, 1790, p. 58.

§ Ibid. 1790, p. 297.

Mr. KLAPROTH says, that by previous experiments he had found, that nitric acid much diluted did not attack the ore when cold; and therefore to separate it from the soluble matrix, he successively poured small quantities of the diluted acid on the ore till all effervescence had ceased, after which the ore was washed and dried. The nitric acid which had been employed was found to contain calcareous earth, and also a considerable quantity of red oxyde of iron, which upon being dissolved in sulphuric acid, left a residuum of lead and siliceous earth.*

Two drachms of the purified ore were mixed with an equal quantity of pot-ash, and afterwards exposed to the fire in a crucible. The mixture melted without intumescence. When cold, the mass was of a reddish colour, and the surface was covered with small scales. Water was poured on it, and the solution was saturated with nitric acid. The following day, the bottom of the glass was found covered with projecting crystals, about a quarter of an inch in length: these crystals were formed of small glittering rhomboidal plates, heaped one on the other. Their flavour was rather metallic. They quickly melted with the blow-pipe, on charcoal, without any increase of bulk, and became small round drops, which were immediately absorbed by the charcoal. When melted by the blow-pipe in a silver spoon, they appeared as small grains, of a greyish colour, which became streaked in cooling, and deposited a white powder during the operation.

When the phosphate of ammoniac and soda was melted, and some of these crystals were added, they speedily dissolved,

* *Analyse Chimique du Plomb Spathique jaune de Carinthie. Annales de Chimie*, 1791, pag. 103.

and communicated to the salt a green colour, more or less deep according to the quantity employed.

They completely dissolved in distilled water, when heated.

Prussiate of pot-ash with this solution produced a reddish-brown precipitate, not very dark.

When some drops of muriatic acid were mixed with the solution of these crystals in water, and a small piece of tin was added, the liquor became of a deep blue.

The solution of muriate of tin poured on the crystals produced the same effect.

Mr. KLAPROTH from these experiments concludes, that the crystals are the acidulous molybdate of pot-ash, especially as the crystals obtained from the filtrated solution of the molybdæna of Altenberg, detonated with nitre, and saturated with nitric acid, have the same properties.

As in the above experiment the ore did not appear to have been completely decomposed, Mr. KLAPROTH mixed two drachms of the purified ore with ten of carbonate of pot-ash, melted the whole in a crucible, and reduced it to powder, and dissolved it in water.

The solution was filtrated, partially saturated with muriatic acid, and heated. A white precipitate fell, resembling curdled milk, which consisted of molybdic acid, and a still larger quantity of oxyde of lead. When dissolved in muriatic acid, the lead was precipitated in the state of muriate of lead.

This precipitate being separated by a filter from the alkaline solution partially saturated with muriatic acid, the solution was then completely saturated with the same acid, and again became slightly turbid, and deposited a white precipitate, which resembled starch in cold water. This precipitate,

after it had been washed and dried, was subjected to the same experiments as the abovementioned crystals, and exhibited the same properties, excepting that it did not dissolve in distilled water till some drops of muriatic acid were added.

When the solution was evaporated in a glass bason, the rest of the oxyde of molybdæna was deposited in the form of a heavy citron-coloured powder.

The white oxyde of lead on the filter through which the solution of the alkaline mass had passed, was found to be mixed with siliceous earth. When melted on charcoal, it did not entirely assume the metallic form, but a part changed into a small grain of transparent yellowish vitrified oxyde of lead. Mr. KLAPROTH observes, that in this experiment the presence of the siliceous earth prevented the complete reduction of the lead, in the same manner as when glass of lead, composed of three parts of oxyde of lead and one of siliceous earth, is melted upon charcoal. He therefore dissolved this oxyde in weak nitric acid, separated the siliceous earth by a filter, and precipitated the lead by sulphuric acid.

Mr. KLAPROTH, however, doubts whether the greater part of the siliceous earth was not introduced, during the operation, by the action of the alkali on the crucible.

A drachm of the ore was digested with a considerable quantity of nitric acid, and a great part was dissolved. In the solution white flocculi were perceived, and were separated by a filter. When dried they were like a membrane, which became bluish when exposed to the light, and much resembled the molybdic acid obtained from molybdæna, by repeatedly distilling nitric acid from it. In the filtrated nitric solution there was much oxyde of molybdæna mixed with oxyde of

lead. The lead was therefore precipitated by sulphuric acid, and afterwards the molybdæna by prussiate of pot-ash.

A drachm of the ore was digested with muriatic acid, and was completely dissolved, excepting a small residuum of siliceous earth. The solution was transparent, and without colour. In the course of some time it plentifully deposited crystals of muriated lead. When these crystals were separated, the solution was evaporated, and the interior of the bason was, during the evaporation, covered with a bluish saline crust, which dissolved and the colour disappeared when the vessel was shaken. The concentrated solution decanted from the muriate of lead (which had been precipitated during the evaporation) was of a deep blue, which disappeared when water was added. The solution was then saturated with alkali, and deposited a white precipitate, which consisted of molybdic acid, together with a small quantity of oxyde of lead.

According to these experiments Mr. KLAPROTH remarks, that the yellow sparry lead ore of Carinthia is composed of oxyde of lead and molybdic acid, and that this mineralogical novelty is the more remarkable, as it is the only one of the kind known at present. It is also worthy of notice, that the molybdæna changes its form according to the method employed to precipitate it from alkaline solutions; for it is obtained either in a crystalline form, or in that of a white powder, or in that of a citron-coloured oxyde. When crystallized, it is soluble in acids, and in water; as a white powder it does not dissolve in water, unless a small quantity of muriatic acid is added; but in the state of the citron-coloured oxyde, it is insoluble in water as well as in the acids. Mr. KLAPROTH considers that this difference is occasioned by the presence of

some alkali in the two first substances, so as to form an imperfect neutral salt with the molybdic acid, but that the yellow powder is in the state of a simple oxyde.

This yellow colour probably occasioned the supposition that the lead was mineralized by the tungstic acid; but the blow-pipe is sufficient to distinguish them. The yellow oxyde of molybdæna loses the colour as soon as the point of the flame touches it, inclines to an olive colour, and melts into small grains, which are immediately absorbed by the charcoal. In the phosphate of ammoniac and soda it dissolves, and communicates to it a green colour. On the contrary, the yellow oxyde of tungsten by ignition becomes blue or black, remaining refractory, and with phosphate of ammoniac and soda it produces a sky-blue glass.

Mr. KLAPROTH concludes his paper by saying, that he could not exactly determine the proportions of the ingredients, as the quantity of the ore in his possession was not sufficient to make the necessary allowance for the solubility of the oxyde of lead in the alkalies, and especially that of the molybdic acid when in a state of combination.

These experiments of Mr. KLAPROTH certainly prove that this ore is a molybdate of lead; but as the quantity which he had was too small, either to make a greater number of experiments, or a regular analysis, I was induced to attempt a further investigation of it; and therefore in the course of the last summer I made the experiments and analysis which are described in this paper.

§ II. *Characters of the* Carinthian *Molybdate of Lead.*

The molybdate of lead is found at Villach, in Carinthia.* The matrix is a limestone, of a pale brownish-grey colour, often more or less tinged with oxyde of iron.

The ore is a heavy brittle substance, easily scratched with a knife, and of a yellow, varying from pale yellow to orange colour.

The fracture is sparry.

The external lustre is like that of wax, and when crystallized, two of the faces of the crystals are commonly opaque, and of a pale yellow, but the remaining four faces or sides have a resinous appearance.

It generally exhibits an appearance of crystallization, and the crystals, when perfect, afford various modifications between the octoëdral figure and the cube.

The specific gravity of a specimen, from which I had separated all the visible part of the matrix, was 5092 (the temperature of the water being 60°), but when the ore was reduced to powder, and purified by diluted nitric acid, I found the specific gravity to be 5706.

1. When the ore was examined by the blow-pipe, it at first split and crackled as soon as the point of the flame touched it, but afterwards readily melted into a dark coloured mass, in which were some shining globules of lead.

2. With borax it formed a brownish-yellow globule, but when it was in a small proportion, and heated by the interior

* It is said to have been sometimes found in Austria and Hungary, but I doubt if the nature of these ores is the same.

flame, it occasionally produced a glass, which was greenish blue, and sometimes deep blue.

3. With phosphate of ammoniac and soda it formed a sea-green glass, which in proportion to the quantity of the ore sometimes became deeper in colour, so as to be nearly of a deep blue.

Before I made the following experiments, I reduced eight ounces of the ore to a fine powder, and dissolved the matrix after the manner of Mr. KLAPROTH, by successively pouring on the powder small quantities of nitric acid diluted with six parts of distilled water, after which I edulcorated and dried the residuum. The nitric acid used in this operation contained (as Mr. KLAPROTH has mentioned) calcareous earth, oxyde of iron, and oxyde of lead; but as prussiate of pot-ash produced a pale green precipitate, I suspected that some other metallic substance beside iron and lead was in the solution. I therefore added muriate of tin to a portion of it, which was immediately changed from a pale yellow to a pale blue, and shewed that a small quantity of molybdic acid was present in the solution.

§ III. *Molybdate of Lead with Water.*

I boiled twelve ounces of distilled water on twenty grains of the purified ore in a glass matrass during three hours. The ore did not appear to be changed, nor did the water after it had passed the filter afford any trace of matter in solution. I believe, therefore, that the molybdate of lead is insoluble in water.

§ IV.

As Mr. KLAPROTH had proved the action of the fixed alkalies on the molybdate of lead, in the dry way, I was desirous to know what effects they would produce in the humid way, and therefore made the following experiments.

EXPERIMENT I.

A. I boiled four ounces of strong lixivium of caustic pot-ash with twenty grains of the purified ore, till there remained at the bottom of the matrass a dry mass, which was partly red, yellow, and green. I reduced this to powder, and poured distilled water on it, till the water came away without any taste.

The alkaline solution was filtrated, and afterwards saturated with sulphuric acid. The liquor then became turbid, and deposited a small quantity of a white precipitate, which consisted of lead and some molybdic acid. This was separated by a filter, and prussiate of pot-ash being added to the clear liquor, precipitated a great quantity of molybdæna, in the state of a reddish-brown flocculent precipitate.

B. I took the residuum of the alkaline solution (which now was chiefly of a red colour, and appeared like minium) and poured nitric acid, very largely diluted, on it till the whole was dissolved. I then precipitated the lead with sulphuric acid, and from the clear liquor which remained I afterwards, by the means of prussiate of pot-ash, obtained a quantity of Prussian blue.

EXPERIMENT II.

A. Twenty grains of the purified ore were boiled with four ounces of a lixivium of carbonate of pot-ash. When all the water was evaporated, there remained a white saline mass, which was reduced to powder, and treated with distilled water as in the former experiment.

A large quantity of a heavy white residuum remained on the filter.

The clear solution was saturated as before with sulphuric acid, and a white precipitate, similar to that of the former experiment, was obtained. This was separated, and a copious precipitate of molybdæna was produced, upon the addition of prussiate of pot-ash.

B. The white residuum was then edulcorated, and when diluted nitric acid was poured on it, it was dissolved with effervescence. From this solution I precipitated the lead by sulphuric acid, and afterwards the iron by prussiate of pot-ash.

Ammoniac, when digested on the ore, had not any effect.

From these experiments it appears, that the molybdate of lead is decomposed by the fixed alkalies in the humid way, and that the component parts of the ore are lead and iron mineralized by the molybdic acid.*

* The alkalies, whether caustic or combined with carbonic acid, do not act in the humid way on molybdæna when mineralized by sulphur. SCHEELE's Essays, p. 230; and *Mémoire sur la Molybdène, par M.* PELLETIER, *Journ. de Physique, Decembre*, 1785, p. 437.

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The alkaline solution was filtrated, and afterwards saturated with sulphuric acid. The liquor then became turbid, and deposited a small quantity of a white precipitate, which consisted of lead and some molybdic acid. This was separated by a filter, and prussiate of pot-ash being added to the clear liquor, precipitated a great quantity of molybdæna, in the state of a reddish-brown flocculent precipitate.

B. I took the residuum of the alkaline solution (which now was chiefly of a red colour, and appeared like minium) and poured nitric acid, very largely diluted, on it till the whole was dissolved. I then precipitated the lead with sulphuric acid, and from the clear liquor which remained I afterwards, by the means of prussiate of pot-ash, obtained a quantity of Prussian blue.

EXPERIMENT II.

A. Twenty grains of the purified ore were boiled with four ounces of a lixivium of carbonate of pot-ash. When all the water was evaporated, there remained a white saline mass, which was reduced to powder, and treated with distilled water as in the former experiment.

A large quantity of a heavy white residuum remained on the filter.

The clear solution was saturated as before with sulphuric acid, and a white precipitate, similar to that of the former experiment, was obtained. This was separated, and a copious precipitate of molybdæna was produced, upon the addition of prussiate of pot-ash.

B. The white residuum was then edulcorated, and when diluted nitric acid was poured on it, it was dissolved with effervescence. From this solution I precipitated the lead by sulphuric acid, and afterwards the iron by prussiate of pot-ash.

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§ V. *Molybdate of Lead with Sulphur.*

A mixture, composed of fifty grains of the ore and 150 grains of sulphur, was put into a small glass retort, to which a receiver was luted.

The fire was then gradually raised till all of the sulphur was driven over, and the bottom of the retort began to melt.

The residuum was a black loose powder, which was greasy to the touch, and soiled the fingers like molybdæna.

This black powder was digested in a strong heat with nitric acid, diluted with three parts of water.

Nitrous fumes were discharged during the digestion, and the powder was dissolved, excepting a residuum of molybdic acid, which was of a greenish-yellow colour.

The solution was diluted with an equal quantity of distilled water, and was filtrated. Sulphuric acid was then added till all of the lead was precipitated; after which I obtained a brown precipitate by prussiate of pot-ash.*

§ VI. *Molybdate of Lead with Carbonate of Ammoniac.*

A mixture, composed of 50 grains of the ore and 220 of dry carbonate of ammoniac, was put into a glass retort, and was sublimed with a gentle heat.

The molybdate of lead remained in the retort without having suffered any apparent alteration.

The ammoniac, however, had raised a small portion; for

* As the quantity of molybdic acid in the ore is much greater than that of iron, it is scarcely possible to discover the latter when they are precipitated together by prussiate of pot-ash.

when it was dissolved in distilled water, and was saturated with an acid, prussiate of pot-ash produced a brown cloud.

§ VII. *Molybdate of Lead sublimed with Muriate of Ammoniac.*

EXPERIMENT I.

A mixture of 50 grains of the molybdate of lead and 240 grains of muriate of ammoniac was sublimed.

The sublimate was partly yellow, green, and blue; there was also some muriatic acid, and the residuum was a black powder.*

A. The sublimate was mixed with an equal weight of sulphur, and distilled.

The residuum of this was a black powder, resembling the mineral called molybdæna, and when distilled with nitric acid, afforded a citron-coloured oxyde.

B. A quantity of distilled water was boiled on the residuum of the first sublimation, by which a part was dissolved, and communicated a blue colour to the water.

1. Prussiate of pot-ash added to some of this blue liquor, produced a precipitate of Prussian blue.

2. Sulphuric acid added to another portion deepened the blue colour.

3. Lixivium of carbonate of soda precipitated some ochry matter.

4. And nitrate of silver was decomposed, and muriate of silver was precipitated.

* M. Sage has observed, that molybdæna with muriate of ammoniac affords a blue sublimate. *Journ. de Physique*, 1788, p. 389.

C. Nitric acid diluted with six parts of water was then poured on the undissolved powder, and was digested on it in a sand heat.

The powder was nearly dissolved, and the solution was colourless.

1. From this solution I precipitated sulphate of lead by the means of sulphuric acid.

2. With prussiate of pot-ash I obtained a brown precipitate of molybdæna, and

3. Muriate of tin turned another portion of it blue.

From these experiments it appears that the first solution contained iron, with some molybdic acid dissolved in muriatic acid; and the second solution contained molybdic acid and lead.

Molybdate of Lead sublimed with Muriate of Ammoniac.

EXPERIMENT II.

125 Grains of the ore were mixed with two ounces of muriate of ammoniac, and put into an earthen matrass, to which a head of stone-ware was fitted. The matrass was then exposed to a sufficient degree of heat, and when all was sublimed the vessels were separated.

The black powder which remained was mixed with two ounces of muriate of ammoniac, and again sublimed.

This operation was repeated three times, after which nothing remained in the matrass. The sublimate, as before, was yellow, green, and blue.

A. Distilled water was poured on the sublimate, so as to

dissolve all of the saline part; but as the solution was turbid, it was poured on a filter, which collected a precipitate of a pale bluish-grey colour.

B. This precipitate after it had been edulcorated was boiled with distilled water, which was afterwards filtrated.

1. Prussiate of pot-ash precipitated some iron.

2. Muriatic acid was added to another portion, after which the prussiate produced a brown precipitate of molybdæna.

3. Muriate of silver was precipitated when nitrate of silver was dropped in.

C. I then boiled lixivium of carbonate of pot-ash on the undissolved part of the residuum, by which the greatest part was dissolved. The alkali was then saturated with muriatic acid, and prussiate of pot-ash being added, precipitated some molybdæna.

On the small portion of the residuum which remained I poured diluted nitric acid. The solution was then filtrated, and I obtained a small quantity of sulphate of lead by the means of sulphuric acid.

These experiments shew that the residuum was composed of molybdic acid, iron, lead, and a small quantity of muriatic acid, which was produced from the muriate of ammoniac during the sublimation.

D. I now took the solution A and divided it into two portions, to one of which I added three ounces of concentrated sulphuric acid, and evaporated the liquor to half of the quantity.

When it was cold it deposited a white saline matter, which for the greater part dissolved in water, leaving a small residuum which appeared to be muriate of lead.

Pot-ash expelled some ammoniac from a portion of the dry salt.

And a precipitate was produced when muriate of barytes was added to the solution. This white saline matter was therefore a mixture of sulphate of ammoniac with a small portion of muriate of lead.

The solution to which the sulphuric acid had been added was again evaporated to a considerable degree, and when cold it resembled a very thick mucilage of a pale yellow colour. It readily dissolved in water, and contained sulphuric acid in great excess.

1. Prussiate of pot-ash only changed the colour to pale green.

2. Carbonate of pot-ash expelled the ammoniac, and a white precipitate like starch was formed, which was principally composed of molybdic acid and pot-ash, on which I shall not make at present any observations, as it will be hereafter noticed in several parts of this paper.

It is worthy of notice, that prussiate of pot-ash did not precipitate the molybdæna from this mucilaginous salt, and that when the pot-ash expelled the ammoniac it formed with the molybdic acid a salt more difficultly soluble than that formed by ammoniac.

E. To the second portion of the solution I added three ounces of concentrated nitric acid, and evaporated it nearly to dryness.

A bright yellow matter was deposited, which I found to be molybdic acid combined with a portion of lead.

There was also a small quantity of liquid, which I diluted with distilled water, and then precipitated some sulphate of lead by sulphuric acid. When this was separated I added

prussiate of pot-ash, and obtained a quantity of Prussian blue.

§ VIII. *Molybdate of Lead, with black Flux.*

One hundred grains of the ore were mixed with four times the weight of black flux. The mixture was then put into a crucible with a piece of charcoal over it; a cover was fitted to the crucible, and it was placed in a furnace in which a strong heat was kept up during an hour. When the crucible was cold and was broken, there did not appear any reguline button, but shining metallic particles were dispersed throughout the mass.

It was then reduced to powder, and the largest particles were separated by washing, were dried upon paper, and weighed 31 grains; upon examination they proved to be lead in the metallic state.

Other particles were separated by a magnet, and the remainder consisted of a black powder.*

A. Diluted nitric acid was poured on this black powder and dissolved it, excepting a small residuum, which consisted of siliceous earth with a little charcoal.

1. The solution was diluted with distilled water and filtrated.

2. I then first separated a quantity of lead by sulphuric

* Scheele could not reduce molybdic acid by black flux. Scheele's Essays, p. 238.—Several chemists however assert, that they have obtained a regulus of molybdæna, particularly Mr. Hielm, in *Journ. de Physique*, 1789, p. 372.—And Mr. Ruprecht, *Journ. de Physique*, 1790, p. 231.—But Mr. Islmann did not succeed. *Journ. de Physique*, 1788, p. 296.

acid, and afterwards obtained some Prussian blue by prussiate of pot-ash.

B. The alkaline solution which had been formed when the melted mass was washed, was poured on a filter, and distilled water was added till it came away tasteless.

The filtrated liquor was without colour: nitric acid was then added till the alkali was saturated.

When about half of the requisite quantity of nitric acid was poured in, the liquor became pale blue, and as the quantity was increased it changed to green; and, lastly, when the nitric acid was added till it was in a small excess, the liquor was of a bright amber colour.

1. This solution, with prussiate of pot-ash, afforded a brown precipitate of molybdæna.

2. Muriate of tin changed the colour to a beautiful deep blue.

3. But sulphuric acid had not any effect.

C. The amber-coloured solution was evaporated to dryness, and a saline mass of a bright citron colour remained at the bottom of the vessel.

As part of the yellow colouring matter appeared to be only mixed with the salt, I dissolved it in distilled water, and separated a quantity of a citron-coloured powder, which was the molybdic acid.

The solution was twice again evaporated, and each time some molybdic acid was separated; but a part still remained intimately combined with the salt, so as with water always to produce the amber-coloured solution.*

* Although the solution at first afforded a brown precipitate with prussiate of pot-ash, yet when it had been twice evaporated, it required the addition of some nitric

I now proceeded to examine the ore with the acids.

As the results which I obtained when the ore was digested with nitric acid were the same as those mentioned by Mr. KLAPROTH, I shall not repeat them, but shall only observe, that it does not appear possible to decompose the ore completely by means of this acid.

§ IX. *Molybdate of Lead with Muriatic Acid.*

240 Grains of the purified ore in fine powder were put into a glass matrass, with three ounces of pure muriatic acid.

The matrass was then placed in a sand bath; in about an hour the whole was dissolved, excepting some muriate of lead, which I dissolved by pouring water on it.

After this there only remained a very small residuum of siliceous earth.

A. The solutions were then added together, and formed a liquor which was transparent, and of a greenish-yellow colour.

1. Prussiate of pot-ash produced a copious precipitate of molybdæna, in the form of a reddish-brown flocculent matter.

B. Lixivium of carbonate of pot-ash precipitated a yellowish-white matter, and turned the liquor to a deep blue.

C. Carbonate of soda had the same effect.

D. Solution of carbonate of ammoniac produced a similar precipitate, and caused the liquor to become blue.

acid before any precipitation could be made. The cause of this was, that the first liquor contained nitric acid in excess, which was afterwards expelled by the subsequent evaporations.

These precipitates were separately collected and washed on filters.

When examined by the blow-pipe, all of them afforded a yellowish-green glass, with phosphate of ammoniac and soda.

These precipitates dissolved in diluted nitric acid with effervescence, and sulphuric acid precipitated sulphate of lead, after which Prussian blue was precipitated by prussiate of pot-ash, and the liquor became brown.

E. The blue solution, which consisted of the muriatic and molybdic acids combined with soda, was evaporated.

When the liquor became hot, the colour changed from blue to pale yellow, and the evaporation was continued without any other perceptible alteration till the whole was become a dry concrete salt.

I dissolved this salt in distilled water, and added muriatic acid, so as to be in a small excess. The liquor was then evaporated to half, and was set in a cool place.

The following morning I found a quantity of crystallized muriate of soda at the bottom of the bason, covered with a white flocculent precipitate, which I collected and edulcorated on a filter. The rest of the liquor was repeatedly evaporated, till I had separated the greatest part of this white matter from the muriate of soda.

The last portion of the liquor, however, still contained some molybdic acid, combined with the muriate of soda; for after it had been several times evaporated and again dissolved, it became blue when muriate of tin was added; or if muriatic acid was first poured in, prussiate of pot-ash produced a reddish-brown precipitate of molybdæna.

Experiments on the White Precipitate.

1. It was not dissolved when water was boiled on it.

2. When digested with sulphuric or muriatic acid, the greatest part was dissolved, and prussiate of pot-ash produced a precipitate of a greenish-brown colour.

3. A small part became yellow when nitric acid was distilled from it.

4. The solutions of carbonate of pot-ash, soda, and ammoniac, dissolved the greater part; and when these solutions were saturated with muriatic acid, prussiate of pot-ash produced precipitates like those of the acid solutions.

As this precipitate in a great measure resembles that which is described in § X. I shall, when I come to that part, give a more ample account of it.

F. I next examined the blue solution, which consisted of the muriatic and molybdic acids combined with ammoniac.

It was first filtrated, and then gradually evaporated. When evaporated to half of the original quantity, the colour was green, but towards the end of the operation it again became blue, and when evaporated to dryness, the residuum was a whitish salt, tinged in some parts with blue.

G. This salt was reduced to powder, and was put into a small glass retort, to which a receiver was fitted. I then placed the retort in a small open furnace, and gradually raised the fire till the bottom of it began to melt. The retort was now removed, and I examined the contents. The receiver contained some water, and a small quantity of muriatic acid.

Near the extremity of the beak of the retort was some muriate of ammoniac, with some fuming muriatic acid, and the

remainder of the tube was filled with a hard greyish-blue salt. In the retort was a black pulverulent residuum.

I collected all of the blue salt, and again sublimed it, and again obtained muriatic acid, blue salt, and some of the black powder.

The blue salt was composed of muriate of ammoniac combined with the acid, or rather with a blue oxyde of molybdæna.

H. The black residuum was put into a glass retort, and some nitric acid being poured on it, it was exposed to a moderate heat. Nitrous fumes were discharged, and when the distillation had been repeated, I found the whole of this black powder converted into the citron-coloured molybdic acid.

I had evident proof that in this experiment a portion of the muriate of ammoniac was decomposed by each sublimation, and also that part of the molybdic acid was deprived of oxygen, and remained in the retort, if not in the state of metal, at least combined with so small a quantity of oxygen as to be nearly approaching to it.*

Molybdate of Lead with Muriatic Acid.

EXPERIMENT II.

One drachm of the ore was digested with muriatic acid, and distilled water was added till the whole was dissolved, excepting a small residuum of siliceous earth.

The solution was filtrated, and was repeatedly evaporated, till muriate of lead was no longer separated.

* A black powder of a similar nature appears to have been obtained by SCHEELE, when he distilled the white molybdic acid with a small quantity of olive oil. Essays, p. 238.

The muriate of lead, when edulcorated, I found to be perfectly free from any other substance.

I now saturated the acid solution (from which the muriate of lead had been separated) with solution of carbonate of ammoniac, and obtained a pale yellow flocculent precipitate, which was well edulcorated.

This precipitate immediately dissolved in very dilute nitric acid, and with sulphuric acid I precipitated a small portion of lead, after which, with prussiate of pot-ash, I separated a quantity of iron.

The solution, when saturated with the ammoniac, was deep blue, and was composed of muriatic acid, ammoniac, and the blue oxyde of molybdæna, like that mentioned in the former experiment.

In the course of these experiments, I have observed that the full blue colour only takes place at the precise moment of saturation, and if the alkali is even added to a considerable excess, the colour does not suffer any farther change; but if much water is first added, the blue colour does not appear; or if water is added afterwards, the solution becomes colourless.

By the preceding experiments the component parts of the ore are proved to be lead, iron, and molybdic acid.

In respect to the small portion of siliceous earth, it may rather be regarded as accidentally present, than as an essential ingredient.

My second object was to discover the least exceptionable mode of analysis, but all of those which have been described appeared to me to be liable to considerable objections, and I was therefore induced to make some additional experiments.

§ X. *Molybdate of Lead with Sulphuric Acid.*

From the experiments of Mr. KLAPROTH it appears, that he only employed nitric and muriatic acid; and on this account I was desirous to try the effects of sulphuric acid, especially as SCHEELE has proved that it is able to dissolve a considerable quantity of the acid of molybdæna.* Moreover, I conceived hopes of success, on the supposition that the elective attraction between lead and sulphuric acid was more powerful than that between lead and molybdic acid; and as sulphate of lead is nearly insoluble, I should thus be able to make a complete separation of the constituent parts of the ore, I therefore made the following experiment.

120 Grains of the purified ore in fine powder were put into a small glass matrass, and one ounce of concentrated sulphuric acid was poured on the powder.

The matrass was then placed over an ARGAND lamp, and the acid was boiled during an hour. The ore became white, and the acid was of a beautiful blue.

When it had stood some hours, I poured the acid into another vessel, edulcorated the powder with distilled water, and added the washings to the acid solution.

Another ounce of sulphuric acid was poured on the powder, and boiled as before. It was faintly tinged with blue; and I added it to the first solution, as well as the water with which the powder was washed.

That I might not leave any part of the molybdic acid unextracted, I repeated the operation a third time. The powder

* Essays, p. 235.

was then edulcorated, and the acid solutions being added together, formed a pale blue transparent liquid.

To decompose the white residuum, I boiled it during an hour with lixivium of carbonate of soda, then washed the powder, and poured on it nitric acid much diluted, by which it was immediately dissolved with effervescence, leaving a very small residuum of siliceous earth.

Prussiate of pot-ash produced a white precipitate of lead, and after 12 hours, a very small portion of Prussian blue subsided.

The ore was therefore decomposed, and there only remained this minute portion of iron to be separated from the lead; but this I shall notice when I come to the analysis.

I now diluted the sulphuric solution with a large quantity of distilled water, and divided it into two portions.

One of these (which I shall call A) was saturated with carbonate of pot-ash. The other (B) was saturated with carbonate of ammoniac.

When the alkalies were first added, the solutions became of a pale yellowish green, and as the quantity of the alkalies was increased, the solutions changed to bluish green, and lastly to deep blue.

When the vessels had stood about 24 hours, the solution A deposited a white precipitate, and in the solution B there was also a small quantity of a yellowish colour.

Both solutions passed the filter without any diminution of the blue colour.

I collected the white precipitate of A on a filter, and then evaporated the liquor to half.

The following morning I found a large quantity of crystal-

lized sulphate of pot-ash covered with a flocculent white precipitate. This I separated from the sulphate of pot-ash, and by repeated evaporations obtained the remainder.

These white precipitates were kept separate in the order in which they were obtained, and were washed and dried.

Experiments on the first Portions of the White Precipitate obtained by the Addition of the Alkali, and by the first Evaporation.

The white precipitate, when exposed on charcoal to the blow-pipe, melted into a pale green transparent globule.

With borax it formed a yellow transparent glass when hot, which as it cooled became opaque, and of an ash colour.

When added to the phosphate of ammoniac and soda in fusion, a yellowish green transparent glass was produced.

Water boiled for a considerable time on the precipitate did not apparently dissolve it; but muriate of barytes shewed the presence of a small quantity of sulphuric acid.

Nitric acid distilled to dryness from some of the precipitate, produced very little alteration, excepting that a small part became yellow.

Sulphuric acid was digested on a portion of the precipitate, and dissolved part of it; the remainder appeared unchanged.

The solution was diluted with distilled water, and prussiate of pot-ash being added, produced a blue precipitate.

Muriatic acid digested on another portion, turned it at first yellow, but afterwards the greater part became white, and remained undissolved.

The solution afforded a blue precipitate with prussiate of

pot-ash, and a small quantity of white precipitate with muriate of barytes.

The residuum was not affected when afterwards digested with acids or alkalies.

Some of the white precipitate was boiled with lixivium of carbonate of pot-ash, and part was dissolved. The solution was then saturated with muriatic acid, and prussiate of pot-ash produced a blue precipitate.

With carbonate of soda the effects were the same.

A solution made with carbonate of ammoniac (afterwards saturated with muriatic acid) was tinged green by the addition of prussiate of pot-ash.

A mixture, composed of 20 grains of the precipitate, and 50 grains of sulphur, was put into a small glass retort, and distilled till all of the sulphur was driven over.

The powder in the retort was of a pale grey colour, did not stain the fingers, or feel greasy, and had an hepatic smell.

Diluted nitric acid was digested on it without heat, and when prussiate of pot-ash was added to a part of the clear solution, a brown cloud was produced in about 10 minutes, resembling the usual precipitate of molybdæna.

Lixivium of carbonate of pot-ash added to another portion, formed a white flocculent precipitate, like starch.

Concentrated nitric acid was afterwards digested on the grey powder in a strong heat, and dissolved a considerable part.

With this solution, prussiate of pot-ash produced a brown precipitate, intermixed with some blue particles.

The rest of the solution was then evaporated, and left a bright yellow mass at the bottom of the matrass.

The undissolved residuum of the nitric solution was then boiled with lixivium of pot-ash, which was afterwards saturated with muriatic acid, and became tinged with blue when prussiate of pot-ash was added.

The residuum was now a black powder, which was edulcorated, and was immediately dissolved when nitric acid was poured on it; at the same time nitrous fumes were emitted. The solution was transparent, excepting that a few white particles were floating in it.

It was then diluted, and filtrated.

Prussiate of pot-ash turned it to a brownish-green, which afterwards became brown. Lixivium of pot-ash precipitated a white flocculent matter; and caustic ammoniac, added to a third portion, precipitated a quantity of iron.

As this precipitate had some remarkable properties, particularly in respect to the difficulty with which it was decomposed, I have been induced to mention the experiments made on it in a circumstantial manner.

This precipitate appears from these experiments to be principally composed of iron, and some molybdic acid, together with a small portion of alkali and sulphuric acid.

The intimate union between the iron and molybdic acid is apparently the cause which impedes the decomposition of this precipitate; but this can only be ascertained by future experiments on molybdate of iron.

I next examined the white precipitate which was deposited by the last evaporations, and found that (when distilled with nitric acid) it was converted into the yellow molybdic acid; and I was therefore convinced that this last portion of the

white precipitate was the same neutral salt which I obtained in several other operations, and which has also been noticed by SCHEELE and KLAPROTH.*

I now began to examine the blue solution B, which consisted of the sulphuric solution of molybdic acid, saturated with ammoniac; but that the experiments made on this may be better understood, I shall first give an account of some experiments and observations which I have made on the sulphate of ammoniac.

Experiments and Observations on the Sulphate of Ammoniac.

This neutral salt, which from GLAUBER (who first prepared it) was called the secret ammoniacal salt of GLAUBER, or vitriolic ammoniac, has been very little examined; neither has it been applied to any useful purpose, although the inventor much recommended it in metallurgical operations.

It has been long known that the fixed alkalies, lime, and barytes (when triturated with it), decompose it, by uniting with the acid. But the effects of heat on it do not appear to have been sufficiently observed.

MACQUER says, that it is (to use his expression) demivolatile, that it may be sublimed entire, and that it cannot be decomposed in close vessels without some intermediate substance.†

BAUME' makes use of nearly the same expressions.‡

* SCHEELE observes, when molybdæna was detonated with nitre, and the mass afterwards dissolved in water, and saturated with sulphuric, nitric, or muriatic acid, that a white precipitate was produced, which was the acid of molybdæna combined with a portion of alkali. Essays, p. 231, and 240.

† *Dictionnaire de Chimie*, Tom. I. p. 111.

‡ *Chimie Expérimentale et Raisonnée*, Tom. II. p. 83.

BUCQUET says, that when it has lost the water of crystallization, it becomes red hot, and melts without being volatilized.

Lastly, M. FOURCROY mentions it in the following manner: "As it contains much water of crystallization, it immediately liquefies by a very moderate heat; but by degrees it "becomes dry, in proportion as the water of crystallization is "dissipated. In this state it first becomes red hot, and soon "melts without being volatilized, according to BUCQUET; but "M. BAUME' asserts that it is demi-volatile. In repeating "this experiment I have observed, that in fact a part of this "salt is sublimed, but a *portion remains fixed in the vessel*, and "without doubt it is concerning this that BUCQUET speaks."*

When so many eminent chemists concurred in nearly the same assertion, I was not a little surprised to observe, in some experiments (which I made for a very different purpose), that the whole of the sulphate of ammoniac was not only volatilized by heat, but also that the distillation of it was accompanied with some remarkable phænomena.

I therefore diluted some very pure concentrated sulphuric acid with an equal quantity of distilled water, and having saturated it with ammoniac, I gradually evaporated it till it became a concrete salt.

EXPERIMENT I.

I put two ounces of the salt into a glass retort, capable of containing three times the quantity, then fitted on a receiver without any luting, and placed the retort in a small open furnace over some lighted charcoal

The salt in the retort speedily liquefied, and a small portion

* *Elémensd'Histoire Naturelle et de Chimie*, Tom. II. p. 93.

of water first came over; this was succeeded by a considerable quantity of alkaline gas, which continued to be produced during 15 or 20 minutes. On a sudden the vessels were filled with a thick white cloud, which upon close inspection appeared to be composed of very minute glittering crystals. This cloud quickly disappeared, and was followed by a great quantity of sulphureous gas and water, the greatest part of which was condensed in the receiver; and the operation went on in this manner while any thing remained in the retort.

During the distillation, the matter in the retort was always liquid; and when the operation was finished, I found in the receiver a considerable quantity of sulphureous acid, with some ammoniac in solution; and in the neck of the retort there was sublimed a portion of the undecomposed salt.

From this experiment I was in a great measure convinced, not only that the neutral salt was decomposed, but that the ammoniac was also in part resolved into its constituent principles.

EXPERIMENT II.

That I might, however, remove any doubt respecting this matter, I fitted a bent glass adopter to a retort, and to this added a double tubulated receiver, from which proceeded a bent tube, which passed under a glass jar filled with water and inverted.

The former experiment was now repeated with this apparatus; and I had the satisfaction to observe, that when the sulphureous acid began to be produced, a quantity of gas at the same time displaced the water in the jar, and continued to pass into it to the end of the operation.

This gas I afterwards examined, and found that it possessed all the properties of the azotic gas.*

I afterwards distilled 100 grains of the yellow oxyde of iron, mixed with 200 grains of sulphate of ammoniac.

Pure ammoniac first came over, and afterwards some sulphureous acid. When the retort began to melt I removed it, and found the iron chiefly in the state of the red oxyde, or colcothar, mixed with some sulphate of iron.

When oxyde of zinc was used, the residuum was sulphate of zinc.

Minium, when triturated with sulphate of ammoniac, immediately decomposed it like lime, or the alkalies, and when distilled, the retort contained sulphate of lead.

When native green oxyde of copper was distilled with sulphate of ammoniac, the residuum was partly red oxyde of copper, with some sulphate of the same. But the ammoniac came over in a concrete state, by reason of the carbonic acid contained in the green oxyde.

The oxydes therefore of iron, zinc, lead, and copper decompose the sulphate of ammoniac by combining with the acid.

I next mixed it with the yellow tungstic acid; but after the distillation, I found the tungstic acid unchanged, excepting that it had acquired a tinge of pale green. The ammoniac

* This operation requires to be conducted with caution; for at the moment when the white cloud appears, a vacuum takes place, occasioned by the alkaline gas (which previously filled the vessels) being neutralized by the sulphureous gas, which is then produced. It is necessary, therefore, in about 10 or 15 minutes after the commencement of the operation, that the fire should be raised, and the azotic gas will then soon begin to pass into the jar. Some water will most commonly rush into the receiver, but if the capacity of this is not too small, there will not be time enough for the water to rise sufficiently high, so as to pass into the retort.

and the sulphureous acid also came over in the same manner as when only the sulphate of ammoniac was distilled.

Lastly, I distilled one ounce of the sulphate of ammoniac with 20 grains of the yellow molybdic acid. During the distillation, the ammoniac and sulphureous acid were produced in as great quantities as when the sulphate of ammoniac was distilled by itself. But the molybdic acid remained in the retort, deprived of oxygen, in the form of a black blistered matter, which was again converted into the yellow acid when distilled with nitric acid.

From these experiments it appears, that the sulphate of ammoniac is not (as many eminent chemists have imagined) incapable of being decomposed without some intermediate substance, but on the contrary, the whole of it can be raised, and a great part decomposed, whenever a proper degree of heat is applied; for then a certain portion of ammoniac first comes over, so that the remainder is combined with acid in excess, and the hydrogen of the ammoniac which remains unites with part of the oxygen of the sulphuric acid, and forms water, which passes into the receiver, accompanied by the acid (now become sulphureous acid), and by the azote in the state of gas.

Various methods have long been in use to decompose ammoniac.

Metallic oxydes produce this effect; and SCHEELE particularly mentions, that if arseniate of ammoniac is distilled, gas is produced, and the acid of arsenic is reduced to the metallic form, and as such is sublimed.*

* Essays, page 155. The same effects also were produced when acid of arsenic was sublimed with muriate of ammoniac, page 161.

The decomposition of the nitrate of ammoniac is also well known; and I have no doubt but that muriate of ammoniac suffers a similar decomposition in a smaller degree each time that it is sublimed; for whenever I have had occasion to sublime muriate of ammoniac, I have always found some fuming muriatic acid; and from whence could this be produced, but from a portion of the salt which was decomposed during the sublimation.

The distillation of the blue triple salt, composed of molybdic acid, muriatic acid, and ammoniac (§ VII. and IX.), places this in a stronger light; for whenever this salt was distilled, a certain portion of molybdæna was left in the retort deprived of oxygen, and muriatic acid was found in the receiver. Moreover, from several repetitions of this experiment, I am well convinced, that by a great number of sublimations the whole of the molybdæna might have been obtained in the proportion that the muriate of ammoniac was decomposed.

When all these facts are considered, it appears to me more than probable that most, if not all, of the ammoniacal salts suffer different degrees of decomposition whenever they are treated in the dry way.

As the molybdic acid was my principal object, I did not make all the experiments I could have wished on this neutral salt; neither have I as yet exactly determined the proportion of azotic gas produced from a certain quantity.

I have found, however, that the sublimed undecomposed part of the salt amounted to 183 grains when an ounce of the salt had been distilled, and that the liquid in the receiver weighed 145 grains; so that 152 grains had escaped, which

principally consisted of azotic gas, together with some sulphureous acid, and some alkaline gas, which had made their way out of the vessels during the operation.

Continuation of the Experiments on the Molybdate of Lead.

From the effects which I observed to be produced when sulphate of ammoniac was distilled with molybdic acid, I was induced to examine in a similar manner the blue solution B; but first I collected, washed, and dried the pale yellow precipitate which had been formed when the sulphuric solution of the molybdic acid was saturated with ammoniac.*

This precipitate, when dry, appeared of a deeper yellow, and easily dissolved in muriatic acid.

Prussiate of pot-ash was then added to the clear solution, and precipitated the whole of the dissolved matter in the state of Prussian blue.

The filtrated solution B was now evaporated till it became a dry concrete salt, the colour of which was pale greyish blue.

I collected this salt, and having reduced it to powder, put it into a small glass retort, and having fitted on a receiver, I distilled it in the same manner as was employed with the sulphate of ammoniac.

The products which came over were also the same, and when the bottom of the retort began to be softened by the

* Whenever the solution was sufficiently diluted, I always found that ammoniac precipitated the iron free from any part of the molybdic acid; but if either of the fixed alkalies were used, a portion of molybdic acid was precipitated with the iron in a state similar to the first portions of those white flocculent precipitates, which have been already mentioned in § IX. and § X.

heat, I removed it, and found the residuum to be a black blistered matter.

I then examined the sulphureous acid and sulphate of ammoniac which had risen, but did not find any trace of molybdæna.

I next poured nitric acid diluted with an equal weight of distilled water on the black residuum in the retort, and distilled it.

As soon as the acid began to be warm, nitrous fumes were discharged, and when the distillation had been repeated with a second portion of nitric acid, I found the whole of the black matter converted into a pale citron-coloured substance, which was the molybdic acid.

I shall not here mention the experiments made on this acid, but shall speak of them after I have given an account of the analysis of the molybdate of lead, which I immediately attempted, according to the method pointed out by the foregoing experiments.

§ XI. *Analysis of the Molybdate of Lead.*

I put 250 grains of the purified ore reduced to a fine powder into a glass matrass, and having poured on it one ounce of concentrated sulphuric acid, I digested it in a strong heat during an hour.

When the solution was become cool and had settled, the acid was cautiously decanted from the powder, and distilled water was poured on till it came away tasteless.

The same operation was repeated twice, so that three ounces of sulphuric acid were used.

The acid solutions and washings were then filtrated, and were received in a large glass vessel.

I diluted the pale blue liquor with distilled water, in the proportion of sixteen to one, and afterwards gradually added ammoniac, until it was completely saturated. The liquor then became deep blue, and appeared turbid.

When it had stood about 24 hours, a loose pale ochry precipitate subsided, and was collected on a filter, the weight of which had been noted.*

This precipitate was edulcorated, and afterwards dried with the filter on the flat top of a tin vessel heated by boiling water, after which the weight of the precipitate was 4.2 grains.

The colour of the dry precipitate was yellowish brown, and when dissolved in muriatic acid it was precipitated by prussiate of pot-ash in the state of Prussian blue.

I now poured part of the clear blue solution, which was composed of sulphuric and molybdic acid saturated with ammoniac, into a glass retort, and when about half was evaporated, I continued to add the remainder of the liquor at different times till the whole was become a concrete salt.

I then raised the fire and continued the distillation until all of the sulphate of ammoniac was decomposed or driven over; but as some of the sublimed salt was fixed in the neck of the

* This is one of the many instances which prove the weak affinity between molybdæna and oxygen; for it is well known that pure ammoniac precipitates iron from sulphuric acid, in a state nearly similar to martial ethiops; but in the present case the iron takes a considerable portion of oxygen from the molybdic acid at the moment that the acid menstruum is saturated by the ammoniac, and it is therefore precipitated in the form of a yellowish-brown oxyde, whilst the molybdic acid being thus deprived of so large a quantity of oxygen, is converted into a blue oxyde which remains in solution.

retort, I turned the bottom of it upwards, and poured some distilled water into the neck, so as to wash out the salt; after this I increased the fire until the whole body of the retort was become red hot.*

The residuum in the retort was a black blistered mass, upon which I poured three ounces of nitric acid diluted with an equal portion of water, and having distilled it, I repeated the operation, and thus converted the whole of the black matter into the yellow acid of molybdæna.

When the retort was sufficiently cooled, I cut off the neck and removed the powder, which weighed 95 grains.

I next proceeded to decompose the residuum left by the acid solution in the state of sulphate of lead, and having edulcorated it, I boiled it during an hour with four ounces of lixivium of carbonate of soda, then washed the powder, and poured on it nitric acid much diluted.

The whole was dissolved, excepting a small portion of white powder, which was washed and dried on a filter by the heat of boiling water, and then weighed seven-tenths of a grain.

This, upon examination, proved to be siliceous earth.

I then exactly saturated the nitric solution with lixivium of pure or caustic soda, and having washed and dried the precipitate of lead, I exposed it in a porcelain crucible for a quarter of an hour to an heat rather below red; after which it weighed 146 grains.

As I had found by a former experiment that a small portion of iron remained with the lead, I dissolved the 146 grains in diluted nitric acid, and precipitated the lead by sulphuric acid.

* To be certain that all of the ammoniacal salt is decomposed, it is absolutely necessary that the retort should be made red hot.

The solution was then filtrated, and being saturated with pure ammoniac, I obtained a small quantity of oxyde of iron, which, when dried as before, weighed one grain.

By this analysis, 250 grains of the ore yielded

			Grains.
oxyde of lead	—	—	146
molybdic acid	—	—	95
oxyde of iron	Grs. 4 / 1 (2)	—	5 2
and siliceous earth		—	0 7
		grs.	246 9
the loss was therefore		—	3 1
			250 0

which I am inclined to place principally to the account of the lead, as it is scarcely possible to decompose the sulphate of lead without some loss, occasioned by the action of the alkaline solution.

§ XII. *Experiments on the yellow Molybdic Acid, obtained by the Analysis.*

A. When exposed to the blow-pipe on charcoal, it was melted by the exterior flame, and the sides of the charcoal were covered with small long crystals, which had a metallic lustre resembling silver.*

When the heat was continued the whole was melted, and

* Scheele mentions a similar product obtained when molybdæna was exposed to the blow-pipe. Essays, p. 230.—Also by sublimation. Essays, p. 238.—And *Mémoire sur la Molybdène, par M.* Pelletier. *Journ. de Physique, Decembre,* 1785, p. 439.

for the greater part absorbed by the charcoal, the edges of which became covered with a blue powder.

When melted in a spoon of platina, some yellow powder was deposited near the edges, and a brownish-yellow shining matter was formed, which became streaked in cooling.

With borax it produced a brownish-yellow glass, but if the quantity of molybdic acid was small, the colour was sometimes blue when the globule was heated by the interior flame.

With soda in the platina spoon it formed a brownish opaque matter.

And with phosphate of ammoniac and soda it formed a glass, which (in proportion to the quantity of molybdic acid) varied from a greenish blue to a deep blue.

B. Ten grains of the yellow molybdic acid were boiled with six ounces of distilled water. About three grains were dissolved, and the solution when filtrated was of a pale yellow colour.

It had scarcely any perceptible flavour, but turned litmus paper red.

When prussiate of pot-ash was added to a portion of the solution, no apparent change was effected; I therefore added a small quantity of nitric acid, which produced a copious brown precipitate of molybdæna. The sulphuric and muriatic acids had the same effect, when poured into the solution, either before or after the addition of prussiate of pot-ash.

With muriate of tin it changed to a beautiful deep blue.

Lead was precipitated from solution of nitrate of lead, in the form of a pale yellow precipitate, which was a regenerated molybdate of lead.

Nitrate of barytes rendered the solution slightly turbid, but

I did not find that the precipitate which subsided was soluble in cold water, as SCHEELE has mentioned.*

The solution did not precipitate lime from nitric acid.

C. Ten grains of the yellow molybdic acid were dissolved when digested with an ounce of concentrated sulphuric acid.

The solution as it cooled became blue.†

Prussiate of pot-ash produced a reddish-brown precipitate.

Muriate of tin had not any effect.

When a portion of the solution was distilled to dryness, the yellow molybdic acid was left in its original state.‡

The remainder of the solution was saturated with lixivium of soda, by which the blue colour was heightened, and some white flocculent matter was precipitated.

Prussiate of pot-ash added to part of this saturated solution did not precipitate the molybdæna, until the alkali was again supersaturated with an acid.

Muriate of tin poured into the solution saturated with alkali changed it to a deep blue; but when the alkali was again saturated with an acid the muriate of tin ceased to have any effect.

The white flocculent matter which was precipitated when

* Essays, p. 234.—SCHEELE does not mention the quantity of water which he employed.

† SCHEELE observes that sulphuric acid dissolves a considerable quantity of molybdic acid, and that the solution as it cools becomes blue and thick; but when heated, the colour disappears, and returns again as the liquor grows cold. Essays, p. 235.

‡ M. PELLETIER says, that a small portion of molybdæna is raised by sulphuric acid when distilled with it; but I did not find it so with the molybdic acid.—*Mém. sur la Molybdène, Journ. de Physique, Decembre,* 1785.

the solution was saturated with soda, was edulcorated and heated with nitric acid, by which it was converted into a yellow powder, similar to the molybdic acid which had been dissolved.

D. Ten grains of the yellow molybdic acid, when digested in a strong heat with one ounce of concentrated muriatic acid, formed a pale yellowish-green solution.

Prussiate of pot-ash precipitated the molybdæna.

Muriate of tin had not any effect.

A portion of the solution being distilled to dryness, left a greyish-blue residuum.*

I then saturated the remaining part of the solution with lixivium of pot-ash, by which the blue colour became more apparent, and a much larger quantity of white flocculent matter was precipitated than when soda was employed.

Prussiate of pot-ash did not affect this solution, until the alkali was again saturated with an acid.

Muriate of tin was precipitated by the solution saturated with alkali, highly coloured with blue; but when the alkali was again saturated with an acid, the muriate of tin had not any effect.†

* SCHEELE has made the same observation. Essays, p. 235.

† From the effects produced by muriate of tin on the molybdic acid dissolved in water, in acids, and in alkalies, it appears that it always tends to deprive the molybdic acid of a great part of its oxygen; and when water is the menstruum, the muriate of tin does this effectually; but when the molybdic acid is dissolved in sulphuric or muriatic acid, the muriate of tin has not any effect, because (as I conceive) the oxygen is supplied by the acid menstruum. This seems the more evident from the effects produced by the muriate of tin when the excess of acid is saturated by an alkali.

Lastly, the white flocculent matter was boiled with nitric acid, and became like the molybdic acid before it was dissolved.

E. Nitric acid did not appear to have any effect on the molybdic acid when digested with it.

F. Two ounces of lixivium of carbonate of pot-ash were poured on ten grains of the molybdic acid.

In a few minutes, carbonic acid was gradually expelled, and as the molybdic acid dissolved, a white flocculent matter was deposited.

After it had stood some hours, the clear liquor was decanted from the residuum.

Prussiate of pot-ash did not affect this solution. Some nitric acid was then dropped in, and produced a reddish-brown precipitate, which was redissolved until the acid was in some excess.

Muriate of tin, when added to a portion of the alkaline solution, was precipitated white, but when some muriatic acid was dropped in the precipitate became blue.

The white flocculent residuum, when treated with nitric acid as in the former experiments, was converted into the yellow molybdic acid.

Another portion of the alkaline solution was evaporated to one-fourth, and in proportion as the evaporation advanced, some of the white flocculent matter was precipitated, but I did not obtain any crystals.

G. Two ounces of lixivium of carbonate of soda were poured on ten grains of molybdic acid.

In a few minutes carbonic acid was expelled, and when the

the solution was saturated with soda, was edulcorated and heated with nitric acid, by which it was converted into a yellow powder, similar to the molybdic acid which had been dissolved.

D. Ten grains of the yellow molybdic acid, when digested in a strong heat with one ounce of concentrated muriatic acid, formed a pale yellowish-green solution.

Prussiate of pot-ash precipitated the molybdæna.

Muriate of tin had not any effect.

A portion of the solution being distilled to dryness, left a greyish-blue residuum.*

I then saturated the remaining part of the solution with lixivium of pot-ash, by which the blue colour became more apparent, and a much larger quantity of white flocculent matter was precipitated than when soda was employed.

Prussiate of pot-ash did not affect this solution, until the alkali was again saturated with an acid.

Muriate of tin was precipitated by the solution saturated with alkali, highly coloured with blue; but when the alkali was again saturated with an acid, the muriate of tin had not any effect.†

* SCHEELE has made the same observation. Essays, p. 235.

† From the effects produced by muriate of tin on the molybdic acid dissolved in water, in acids, and in alkalies, it appears that it always tends to deprive the molybdic acid of a great part of its oxygen; and when water is the menstruum, the muriate of tin does this effectually; but when the molybdic acid is dissolved in sulphuric or muriatic acid, the muriate of tin has not any effect, because (as I conceive) the oxygen is supplied by the acid menstruum. This seems the more evident from the effects produced by the muriate of tin when the excess of acid is saturated by an alkali.

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In a few minutes, carbonic acid was gradually expelled, and as the molybdic acid dissolved, a white flocculent matter was deposited.

After it had stood some hours, the clear liquor was decanted from the residuum.

Prussiate of pot-ash did not affect this solution. Some nitric acid was then dropped in, and produced a reddish-brown precipitate, which was redissolved until the acid was in some excess.

Muriate of tin, when added to a portion of the alkaline solution, was precipitated white, but when some muriatic acid was dropped in the precipitate became blue.

The white flocculent residuum, when treated with nitric acid as in the former experiments, was converted into the yellow molybdic acid.

Another portion of the alkaline solution was evaporated to one-fourth, and in proportion as the evaporation advanced, some of the white flocculent matter was precipitated, but I did not obtain any crystals.

G. Two ounces of lixivium of carbonate of soda were poured on ten grains of molybdic acid.

In a few minutes carbonic acid was expelled, and when the

molybdic acid was dissolved, a small quantity of white flocculent matter was precipitated.

The clear solution was then poured from the residuum.

Prussiate of pot-ash did not produce any precipitate until the alkali was saturated with an acid.

The effects of muriate of tin were also the same as those mentioned in the former experiment.

A part of the solution was evaporated to half, and the next morning I found crystals, which although not very distinct, appeared to be in the form of four-sided tables with the angles truncated.

These crystals dissolved in water without leaving any residuum, and (when the solution was saturated with muriatic acid) the molybdic acid was precipitated by prussiate of pot-ash, as in the former experiments.

H. Two ounces of carbonate of ammoniac in solution were poured on ten grains of molybdic acid, which appeared to be more readily dissolved than when the fixed alkalies were employed.

The solution appeared slightly turbid, but very little of it was precipitated.

The effects produced by prussiate of pot-ash and muriate of tin were the same as in the preceding experiments.

When a portion of the solution was distilled to dryness, part of the molybdic acid remained unchanged, but another part was deprived of oxygen, and appeared in the form of a dark grey powder.*

The remaining part of the solution was considerably eva-

* SCHEELE has observed a similar effect.

porated; and the following day I found a striated yellowish mass, which dissolved in water without leaving any residuum. This solution resembled the former in every respect.

§ XIII. *Molybdic Acid with Sulphur.*

In order to remove every doubt concerning the nature of the yellow acid obtained by the analysis, I made the following experiment.

I put 20 grains of the yellow acid and 100 grains of sulphur into a small glass retort, and continued the distillation till the bottom began to melt.

The residuum was a black substance, which was greasy to the touch, stained the fingers black, communicated to them a shining metallic lustre, and had all the other properties of the mineral known by the name of Molybdæna.

I afterwards distilled this black matter with nitric acid, which converted it into a yellow powder, similar in appearance and properties to the molybdic acid which had been originally employed.

§ XIV. *General Observations.*

It has been proved in the course of this paper that molybdate of lead can be decomposed in the humid way by the fixed alkalies, although these have not any effect when boiled with molybdæna mineralized by sulphur.* The state of the molybdæna in the two substances appears to be the cause of this

* Scheele's Essays, p. 230; and *Mémoire sur la Molybdène, par M.* Pelletier. *Journal de Physique*, 1785, p. 437.

difference, for in the former it is oxygenated, but in the latter I conceive it to be in the state of metal.

From the experiments of SCHEELE it also appears, that of all the known acids only two have any effect on the sulphurated molybdæna, and that these two are the nitric acid, and that of arsenic. The latter, however, seems rather to act on the sulphur than on the molybdæna; but the former communicates oxygen to both, so as to convert the one into sulphuric and the other into molybdic acid.

The rapidity with which nitric acid oxygenates molybdæna, even to supersaturation, resembles the effects produced by the same acid on some other metallic substances, particularly tin; for in both cases the acid ceases to act as soon as the supersaturation with oxygen is effected; and on this account the nitric acid is unable to dissolve the molybdic acid.

Before I proceed, I must observe that whenever a solution of the molybdic acid becomes blue, or tending towards that colour, it is a certain sign that the molybdic acid has suffered a diminution of oxygen. A variety of facts which prove this, have been already brought forward in the different experiments contained in this paper; and I shall soon have occasion to mention others.

Sulphuric acid can dissolve a considerable quantity of molybdic acid; and the solution is always more or less of a blue colour according to the quantity which is dissolved; and the blue colour proves that the molybdic acid has parted with a portion of oxygen; but if the solution be heated, the blue colour disappears, and returns again when the liquor becomes cold.*

* If lead or any other metal is present, the blue colour is permanent.

The cause of this I believe to be a change produced by heat in the respective degrees of affinity which prevail between the metallic base and oxygen, and between the base of the acid menstruum and oxygen; so that when the solution is heated, the affinity between the blue oxyde of molybdæna and oxygen is increased, and a portion of oxygen therefore quits the acid menstruum and combines with the blue oxyde, which then becomes molybdic acid; but as soon as the heat is dissipated, the cause of this a gmentation of affinity ceases, and the acid menstruum receives again the portion of oxygen from the molybdic acid, which then returns to the state of a blue oxyde; or if the heat is continued until the solution is distilled to dryness, the residuum is the molybdic acid exactly in the same state as it was before the solution was made, for the continuation of the heat enables it to retain the portion of oxygen requisite to constitute a metallic acid.

I do not therefore believe that the total quantity of oxygen in the solution suffers alteration any further than that it is distributed in different proportions between the two acidifiable bases, sulphur and molybdæna, according to the temperature of the solution.

As the affinity between azote and oxygen is comparatively weak, the metal molybdæna effects a decomposition of the nitric acid, and acquires a sufficiency of oxygen to become molybdic acid. But as the affinity between sulphur and oxygen is greater than that of azote, and also under certain circumstances superior to molybdæna, the latter requires the assistance of heat to be able to retain a full portion of oxygen, and this increase of affinity lasts no longer than during the continuation of the heat.

To corroborate this assertion, it will be proper to consider the effects of muriatic acid on that of molybdæna, especially as the affinity between the radical principle, or base of the muriatic acid, and oxygen is known to be so great, that no chemist has as yet been able to effect a separation of the constituent principles.

It has been mentioned, that molybdic acid when dissolved in muriatic acid also parts with some oxygen, and tinges the menstruum with a green colour. But heat does not enable it to take back the oxygen, for it augments the effects of the muriatic acid, which, when distilled, passes oxygenated into the receiver, and the molybdic acid is converted into a bluish-grey oxyde.*

These effects clearly prove, that heat in this case acts inversely to what it did when the nitric and sulphuric acids were the menstrua. For then the increase of affinity was between molybdæna and oxygen, but here it is in favour of the base of muriatic acid; so that by the continuation of heat, the muriatic acid carries with it into the receiver a certain portion of oxygen which it has taken from the molybdic acid, and the latter is left in the state of an oxyde.

From this it appears that muriatic acid uniformly tends to deprive the molybdic acid of a certain quantity of oxygen, and that heat produces a contrary effect on this solution to that which it did on the one made with sulphuric acid; and heat and cold do not therefore produce a change of colour.

I do not, however, believe that muriatic acid acts thus constantly on all of those metals which can be dissolved by it; on the contrary, there is a muriatic solution which much re-

* *Elémens d'Hist. Nat. et de Chimie, par M. de* FOURCROY, Tome II. p. 439.

sembles the sulphuric solution of molybdic acid in the vicissitudes of colour which it exhibits by heat and cold.

The phænomena which heat produces on the solution of cobalt in muriatic or nitro-muriatic acid, called sympathetic ink, have long engaged the attention of chemists and others, but as yet great difficulties have occurred whenever an explanation has been attempted.

There can be no foundation for the idea which some have had, that the green colour (which characters traced with this solution on paper assume when heated) is caused by a temporary crystallization of the salt, and the disappearance of the colour by a subsequent degree of deliquescence; because any quantity of the liquid becomes green when heated.

The effects caused by heat on the sulphuric solution of molybdic acid have therefore induced me to suspect a similar cause in the muriatic solution of cobalt; and I believe that heat and cold in like manner causes a temporary difference to take place in the proportions of oxygen existing in the acid menstruum and the oxyde; and this is the more confirmed when the acid is expelled by too great a degree of heat, for then the changes of colour are no longer to be observed.

Heat, it is well known, assists the combination of oxygen with the metals, but I do not believe that the abovementioned alternate effects of heat and cold have been as yet investigated.

It is probable that these are not confined to the two instances which have been adduced, although in other solutions they may not be so apparent. The subject is certainly curious, and worthy of the attention of chemists, as it would reflect much light on the solutions of metals in general.

When the sulphuric or muriatic solutions of the molybdic

acid are saturated with pot-ash or soda, they assume a very deep blue colour at the moment of saturation. The molybdæna is not, however, precipitated in the form of the blue oxyde, but for the greater part remains combined with the acid menstruum and the alkali, and thus forms a triple salt in solution, which differs considerably from another triple salt, which is slowly precipitated at the time of saturation in the form of a white flocculent matter, and is composed of the same three ingredients, but contains the oxyde in the largest proportion.

Sometimes a fourth ingredient becomes added to the last mentioned white precipitate; for when iron is present in the sulphuric or muriatic solutions, it is precipitated by pot-ash or soda intimately combined with the other ingredients, and appears to render the decomposition of the precipitate very difficult.

Although the triple salt which is in solution will pass many folds of paper without leaving any residuum, yet it is not permanent; for by repeated evaporations, the neutral salt resulting from the combination of the acid menstruum and the alkali becomes crystallized, and a white flocculent matter is separated, which does not contain iron like that which was precipitated when the acid solution was saturated with the alkali, but can be converted into the yellow molybdic acid by being distilled with nitric acid, which takes from it the small portion of the acid menstruum and the alkali required to constitute the triple salt.

It has already been observed, that nitric acid has not any effect when immediately digested on molybdic acid, but I have found it otherwise when a third substance was present;

and the effects were nearly the same whether this substance was a metal or an alkali.

The portion of molybdic acid which I detected in the nitric acid employed to purify the ore, and Mr. KLAPROTH's experiments made with the same acid, prove the first, and the experiment mentioned in § VIII. is a proof of the latter.

The phænomena which appeared in the last experiment throw some light on the effects produced by nitric acid on molybdæna; for when the sulphuric and muriatic solutions of the molybdic acid were saturated with pot-ash or soda, they gradually changed to yellowish green, and so on to blue, in proportion as the alkali was added; but when nitric acid was added to the alkaline solution, the change of colour was exactly the reverse of the former, for the changes were then blue, green, and yellow, in proportion to the quantity of nitric acid.

The cause of these effects I conceive to be the different degrees of oxygenation of the molybdæna; for when the first portion of nitric acid was added, it rather combined with the alkali than with the molybdic acid, and the latter was therefore in some degree separated with a diminution of the original quantity of oxygen, and consequently appeared as the blue oxyde in solution. After this, the second portion of nitric acid began to oxygenate the blue oxyde, and therefore changed the colour of the solution to green; but the third addition of nitric acid acted immediately on the oxyde, turned the solution yellow, and when assisted by heat, caused a quantity of the yellow molybdic acid to be precipitated. The alkali, however, appears to have impeded the complete separation of the molybdic acid, and retained a part of it together with the nitric acid, so as to form a yellow triple salt.

When the sulphuric and muriatic solutions of the molybdic acid are saturated with ammoniac, triple salts are formed, which are different in their properties from those which have been described; for the triple salts produced by ammoniac are permanent, and do not appear to be decomposed by evaporation.

If iron is present in the acid solution (sufficiently diluted), it is precipitated by ammoniac free from molybdic acid, especially when the menstruum is the sulphuric acid.

The affinity of the molybdic acid with muriate of ammoniac is so great, that by sublimation it even in part quits lead to unite with it, and then forms the blue triple salt, from which the blue oxyde does not separate, but in proportion that it is deprived of oxygen by the gradual decomposition of the ammoniac caused by repeated sublimations.

When the sulphuric solution saturated with ammoniac is evaporated to a proper degree, the triple salt crystallizes in the usual figure of the sulphate of ammoniac, but the colour is bluish green. If, however, the evaporation is continued to dryness, a pale greyish-blue salt is left, and by distillation this salt is decomposed (after the manner of the decomposition which I have noticed in the sulphate of ammoniac), and the molybdæna remains in the form of a black powder, deprived of oxygen.

I think it necessary here to observe, that when molybdæna is not in the metallic state, it appears to me to suffer four degrees of oxygenation. The first is the black oxyde; the second is the blue oxyde; the third is the green oxyde, which (as it seems to be intermediate between an oxyde and an acid) I am inclined to call molybdous acid, according to the distinction made by the new Nomenclature; the last and fourth

degree is the yellow acid, or that which is supersaturated with oxygen.

The affinity between molybdæna and oxygen is but weak, at least in respect to that portion which is required to constitute molybdic acid; for it has been proved, in the course of these experiments, that considerable changes are produced by a very small difference in the proportions of the acids or alkalies, and even by the degrees of heat. SCHEELE and Mr. ISLMANN have observed, that all of the metals, excepting gold and platina, are able in the humid way to deprive the molybdic acid of oxygen, so as to cause it to become blue; but here their effects appear to cease. M. PELLETIER found that a solution of molybdic acid was turned blue when hydrogenous gas was passed through it. Mr. KLAPROTH has also remarked that light, under certain circumstances, changed the colour of molybdic acid to blue. And the effects of light appear in some measure to be connected with the following experiment.

I made a solution of the molybdic acid, by digesting sulphuric acid on molybdate of lead, and diluted it with an equal quantity of water. The solution was filtrated, and I then added a solution of hepar sulphuris until the brownish-red precipitate which was produced began to fall much paler. After this the liquor was filtrated, and was of a pale beer colour.

I placed it accidentally in an open glass jar, on a shelf near a window, on which the sun shone during a great part of the day, and was surprised to observe that in about two days it began to assume a greenish tinge, which gradually became deeper; on the third day it was of a full green, on the fourth it had a tinge of blue, on the fifth the colour was greenish-

blue, and on the sixth day it was changed to a beautiful deep blue.

The solution continued all the time to be transparent; and although the vessel remained four weeks in the same situation, the blue colour suffered no further change.

This solution much resembles that which SCHEELE discovered in the course of his experiments on manganese; but the apparent similar effects, I believe, are produced by opposite causes; for the changes of colour in the alkaline solution of manganese appear to be effected by the absorption of oxygen, but those of the molybdic solution are caused by a diminution of the inherent quantity.

The contrast of the causes which operate on the two solutions becomes the more evident when the effects which acids produce on them are considered; for when a few drops of an acid are added to the solution of manganese, the changes of colour are accelerated, not merely by the neutralization of the alkali and consequent precipitation of the manganese, but (as I conceive) by the accession of oxygen either immediately from the acid, or from the atmosphere, which the manganese is better able to absorb when the disengagement of it from the alkali is thus assisted by the addition of the acid.* On the contrary, if nitric acid is dropped into the molybdic solution, the colour is immediately destroyed, in the same manner as in all the other blue solutions of molybdæna when oxygen is thus presented to them.

The facility with which molybdæna parts with oxygen is evinced not merely in the humid way, for M. PELLETIER found that molybdic acid yielded oxygen to arsenic when

* SCHEELE's Essays, p. 108.

distilled with it, and that the latter was converted into a white oxyde.

The same is also proved by my experiments on various oxydes distilled with sulphate of ammoniac; for the molybdic acid was the only one which could thus be deprived of oxygen, not excepting the tungstic acid, which has been supposed much to resemble that of molybdæna.

XIII. *Observations of the diurnal Variation of the Magnetic Needle at Fort* Marlborough, *in the Island of* Sumatra. *By* John Macdonald, *Esq. In a Letter to* John Crisp, *Esq. F. R. S.*

Read April 21, 1796.

DEAR SIR, Fort Marlborough, 15th March, 1795.

I HAVE now the satisfaction to transmit to you the observations of the diurnal variation of the variation of the magnetic needle, with which you wished to be furnished. A short account of the mode used in laying off the meridian is prefixed. A small building, devoid of iron, was erected at some distance from my house. This building contained the meridian. By repeated observations, I found the sphere of mutual attraction of the needle and iron to be very small; a common-sized key presented to it, did not affect it at a nearer distance than 5 inches. To exclude every influence of iron, I laid aside such as might be about me, previous to the taking of the observations, and took care to remove the padlock and key, at the same time, to a certain distance from the constructure. The hasp and staple made use of for the padlock were of wood; and for the nocturnal observations, I had wooden candlesticks made. I may venture to say, that the observations were taken with the utmost care; and that every requisite precaution was used to exclude errors that might have arisen from a want of minute attention.

The observations contained in pages 346, 347, and 348, were taken by applying the magnet-box three times, each day, to the meridian. This was, I think, accurately effected, by hairs fixed longitudinally on the inside of the box, corresponding with projecting points on the outside. When these covered each other, and the meridian, to the eye looking through a magnifying glass from above, there was a strong presumption that the longitudinal axis and 0° points of the box were on and in the true plane of the meridian. In reading off, the right hand covered the right eye; a magnifying glass, of a power of $2\frac{1}{2}$, was held in the left hand; the upper axis-hair was brought to cover the 0° point; and the eye, hanging over this point, glanced steadily to the left, to read off the variation west from south, as the fine minute-scale was placed on the flat south end of the needle. This observation was taken each time four times, to ensure accuracy. Lest, however, any error might have arisen from not placing the box accurately in the same situation, in taking the three daily observations, two pieces of seasoned wood were fixed with wooden pins, nearly parallel to the meridian, on either side. The magnet-box was applied to the meridian between these, and kept firmly fixed in its position by a number of small wooden wedges, urged gently between the box and the lateral fixtures. The observations contained in page 349 were taken with the magnet thus fixed. A fourth column of observations, at various nocturnal hours, is given in page 349. A corresponding thermometrical observation is attached to each magnetic one. The thermometer was placed in the room on the left side of my hall.

It appears from these observations, that the east variation

diurnal of the variation, increased from about 7 in the morning till 5 in the afternoon, and that it decreased till 7 in the morning. The irregularities which are apparent do not destroy the general evident inference of result, as it is well known that the common increasing or decreasing variation is subject, in the same place, and on the same day, to certain deviations, from latent causes. It appears in general, that such diurnal variation of the variation as had been observed during thunder, is greater than it ought to have been, *cæteris paribus*.

It has been remarked, that heat weakens the magnetic virtue, and that cold strengthens it. Supposing, with the great HALLEY, the existence of 4 magnetic poles, by blending this supposition with the above principle, well ascertained, attempts have been made to account for the diurnal variation of the variation. The south-east magnetic pole being less heated in the morning, either by the sun or by subterraneous fire, than towards noon, and in the afternoon, and being at the same time, by passing through the meridian of Celebes, nearer Sumatra than the south-west magnetic pole, it draws to it in the morning the south end of the magnetic needle more powerfully than the other attracts; and, consequently, the variation diurnal of the variation ought to be, and actually is, less in the morning than in the afternoon. In the progress of the day, the south-east magnetic pole having become heated, and the south-west pole being at the same time less heated, attracts the south end of the magnetic needle more powerfully than the other does; and hence the east diurnal variation of the variation appears greater in the afternoon than in the morning. It is found in Europe, that this diurnal variation of the variation is greater in summer than in winter. This seems to point out

heat acting on magnets in the earth, as its efficient cause. This was first observed in Europe in 1756, by Mr. CANTON; and the results of the foregoing observation being diametrically opposite to his, with similar effects, afford not a small confirmation of the essential part of HALLEY's theory. The sciences of electricity and magnetism (in my opinion intimately connected) are at present in their infancy. We observe effects which we cannot trace up to any satisfactory cause. If observations similar to these, and to those made by Mr. CANTON, could be made in equal latitudes and longitudes, and in corresponding situations in the torrid zone, by collating the results, I think a rationale of this wonderful phænomenon might be obtained.

From the greatness of the angle of dip of the needle, I am led to suppose that the magnetic poles are fixed within the magnetic nucleus far within the earth's surface, and that some of these poles are more powerful in their action than others, from the variation observed in various places of the globe. I shall be happy if these observations (I mean the figured ones) will enable you to elucidate this subject.

I remain, with much esteem, &c.

JOHN MACDONALD.

24th of June. A meridian was drawn by means of the following apparatus. A plate of brass was cast, and reduced to a level surface in a lathe, in which concentric circles were accurately cut on it. A short hollow cylinder was screwed on the centre. This tube received a brass cylinder, pointed at the upper end. An iron-wood post was fixed in the ground. The brass plate was fixed level on the post by means of a gunner's

perpendicular, and strong screws working in two plates, on the principle of the levelling screws of a theodolite. The cylinder was rendered perfectly perpendicular to the level plate, by means of three small adjusting screws on the tube. These screws acting laterally on the brass wire-pointed pin that gave the sun's shadow on the concentric circles, brought it perpendicular over the centre of the plate. A flat small brass ruler was furnished with a socket, which received the point of the shadow-pin. This ruler had a horizontal arm to it, having a semicircle at one extremity coinciding with the tube on the centre of the plate. A pointed wire passed to the plate through the other extremity of the horizontal arm. This wire could be raised or lowered at pleasure by a small screw. It is evident that if this point touched, *delicately*, the plate of brass, in moving it round (by means of the socket above, and semicircle playing round the tube below), the shadow-pin must be perpendicular. If the plate was acted upon evidently *roughly* by the pin in moving horizontally, it is clear the pin must be inclined to that side, and a delicate correction was given by the little brass screws fixed to the tube screwed on the centre. By this contrivance the shadow-pin was fixed exactly perpendicular to the plate.

The box in which the magnet is suspended seems to have come out of the maker's hand unfinished, having on it no hairs through, or parallel to its axis, by which to apply it truly to a meridian. These hairs were fixed above and below the magnet, in a plane perpendicular to the graduated 0° at both ends. Two very fine brass wires were fixed on the outside, on the ends of the box. These brass wires were in the plane of the hairs, and when applied to the meridian, the hairs

appeared to cover the meridian, through circular holes in the bottom of the box. I may venture to assert that the meridian was laid off within one minute of a degree, as it bisected the chords of 12 different arcs of concentric circles made use of. I may also say that the axis, or 0° line of the box, was applied to the meridian within one minute of true position. In laying off the meridian, the operations of two successive days perfectly agreed.—W. or E. put over each observation, indicates west or east variation. As bad weather may prevent observing at the specified hours at the heads of the columns, the hour, if different from 7 A. M. 12 or 5 P. M. or from others at the heads of columns, will be marked over those particular observations.

N. B. In the weather columns, to save trouble, F. means fair, A. after, R. rainy, N. night, L. lightning, C. cloudy, S. sky, T. thunder, m. much, O. overcast, s. serene, W. windy, l. little.

1794.	Time. 7 A.M.	Thermometer.	Time. 12.	Thermometer.	Time. 5 P.M.	Thermometer.	1st Observation. Weather.	2d Observation. Weather.	3d Observation. Weather.
	E.		E.		E.				
	6	°		°		°			
June 27	1 8	74	1 10	81	1 12	81	F. A. a R. N. C.	a north-west wind	R. O. and W.
28	1 8	78	1 10	78	1 12	80	m. R. in the N. and some L.	C. sun at intervals	a l. C.
	8		2½						
29	1 8	76	1 10	83⅓	1 11	82	C. - -	s. sunshine	C. and sultry
			2						
July 1	1 7	76	1 11	84	1 11	82	s. sunshine	west wind, sunshine	s. wind north-west
2	1 9	76	1 10	82	1 12	82⅓	a clear morning	s. sunshine	s.
3	1 9	78	1 11	82⅓	1 9	82	a clear morning	s. ditto, wind west	s.
4	1 9	74⅔	1 7	82	1 8	82	a l. C.	s. sunshine	a l. C.
5	1 6	76	1 7	81⅔	1 10	82	s. - -	s. ditto - -	s. and sunshine
6	1 9	76	1 11	82	1 12	81½	C. - -	s. sunshine	C. R. and distant T.
7	1 9	76	-	-	1 10	81	C. - -	- - -	s. sunshine
8	1 9	77	1 10	82	1 10	82	a l. C. - -	s. sunshine	s. ditto
9	1 6	78	1 9	82	1 10	82	a s. S. - -	s. ditto -	s. ditto
10	1 6	75	1 10	81⅔	1 10½	83	a s. S. - -	s. ditto - -	s. ditto
11	1 9	76	1 9	82	1 10	84	C. and O.	s. ditto - -	s. ditto
full moon. 12	1 9	76	1 10	82⅓	1 8	83½	a s. S. - -	s. ditto -	s. ditto
13	1 9	75	1 10	83⅔	-	-	a s. S. - -	s. ditto	
14	1 9	78⅓	1 10	83⅔	1 8	84⅓	a clear morning	s. ditto -	s. ditto
15	1 9	79	1 9	84	1 9	84	a ditto - -	ditto - -	s. ditto
16	1 9	74⅓	1 8	83	-	-	a calm clear morning	s. ditto	
18	1 8	76	1 9	82	1 10	81	C. - -	s. ditto -	distant thunder, C.
19	1 9	73⅔	1 10	83	1 9	82	s. sunshine -	a l. C. - -	R. A. T.
21	1 8	79	1 9	83	-	-	s. south-east wind	C.	
22	1 9	74	1 11	77	1 10	79	C. and R. -	C. and R. -	C.
23	1 9	76	1 9	83	1 9	80	C. - -	s. sunshine	C. and R.
24	1 9	74	1 9	82	-	-	s. sunshine -	s. ditto -	
25	1 9	75⅔	1 10	83	-	-	s. ditto - -	s. ditto -	
	8								
new moon. 26	1 9	77	1 7	84	1 9	84½	s. ditto -	s. ditto -	s. sunshine
27	1 8	74⅔	1 10	84	1 9	83½	s. ditto - -	s. ditto - -	s. ditto
28	1 8	75⅔	1 10	83	1 8	82⅓	C. - -	s. ditto -	s. ditto
30	1 9	72½	-	-	-	-	s. sunshine		
31	-	-	1 9	82	1 8	82	- - -	s. ditto - -	s. ditto

1794.	Time. 8 A. M.	Time. 12.	Time. 4 P. M.	Thermometer.	Ditto, 2d observation.	Ditto, 3d observation.	1st Observation. Weather.	2d Observation. Weather.	3d Observation. Weather.
	° ′	° ′	° ′	°	°	°			
	E.	E.	E.						
Aug. 1	1 7	1 8	1 9	77½	84	82	s. sunshine	s. sunshine	C. Thunder
2	1 8	-	-	77½	-	-	s. ditto -		
3	1 6	1 8	1 9	79	82½	82	s. ditto - -	s. ditto -	C.
4	1 7	1 8	-	-	82	-	s. ditto -	s. ditto -	
6	1 7	1 8	1 7	78	83½	83	s. ditto -	s. ditto -	C. Thunder
7	1 8	1 8	1 8	80	82	81	s. ditto - -	s. ditto - -	R. C. and distant T.
8	1 8	1 8	1 8	75	77	78½	C. A. m. R. in the N.	C. and O. -	C.
9	1 8	1 6	1 8	76	82	82	s. sunshine	s. sunshine -	s. sunshine
10	1 6	1 6	-	76	82	-	C. - -	s. ditto -	s. ditto
12	1 6	1 6	1 9	78	82	82	s. sunshine	C. - -	C. and T.
13	1 8	1 6	1 9	78	82	82	s. ditto -	distant Thunder	C. R. and T.
15	1 7	1 7	1 10	79	81	81	s. ditto - -	s. sunshine -	R. and C.
16	1 8	1 8	1 7	77	81	79	C. - -	s. ditto -	m. R.
18	1 8	1 9	1 8	78	81½	80	C. -	C. - -	C. and high wind
20	1 9	1 6	1 8	77½	80	82	C. - -	C. a l. - -	s. sunshine
21	1 8	1 7	1 9	78	83	83	s. sunshine -	s. sunshine -	s. ditto
22	1 7	1 8	1 10	76	78	78½	C. and R. -	C. R. -	C.
23	1 8	1 6	1 9	76	80	78½	C. -	C. a l. - -	s. sunshine
25	1 7	1 7	-	77	81½	-	s. sunshine -	s. sunshine -	C. distant T.
30	1 7	1 8	1 9	76	82	81	a l. C. - -	s. ditto -	C. distant T.
31	1 8	1 6	1 8	77	82	79½	F. A. R. -	C. - -	s. A. R.

N. B. It was, from forgetfulness, omitted to take and insert the thermometrical observation which should have been attached to the morning variation of the 4th. It is, however, a matter of little consequence in the general consideration of the subject.

1794.	Time. 9 A.M.	Thermometer.	Time. 12.	Thermometer.	Time. 3 P.M.	Thermometer.	1st Observation. Weather.	2d Observation. Weather.	3d Observation. Weather.
	E.	°	E.	°	E.	°			
October 1	1 8	80	-	-	1 9	80¼	s. sunshine	R. A. C. S.	
2	1 6	77	1 8	82	1 8	83	s. A. R. -	A. l. C. -	s. sunshine
4	1 6	78	1 9	82	-	-	s. sunshine	s. sunshine	
5	1 6	80	-	-	1 8	84	a l. C. -	- -	ditto
8	1 6	78	1 10	83½	1 10	84	s. sunshine	s. sunshine	s. ditto
9	1 6	79	-	-	-	-	ditto		
10	1 6	78	1 9	81	1 10	83½	C. - -	s. ditto -	distant T. sunshine
12	10 1 6	79	1 8	82½	1 9	82½	s. sunshine	s. ditto -	C. distant T.
14	1 8	79	-	-	-	-	s. ditto		
15	-	-	1 8	82½	-	-	- -	s. ditto	
16	1 7	80	1 7	83	1 7	84	a l. W. -	s. ditto -	sunshine and W.
17	1 6	79½	1 7	83	1 9	84	s. sunshine	a l. C. -	ditto s.
18	1 5	78	1 8	81½	1 8	80¼	C. - -	s. sunshine	C.
20	-	-	1 10	83	-	-	- -	s. ditto	
21	10 1 8	80	1 9	84¼	1 10	85	s. sunshine	s. ditto -	distant T. s.
22	1 6	78	1 8	84	-	-	s. ditto -	s. ditto a l. W.	
23	1 5	77½	1 9	83	1 9	83	F. A. R. -	s. ditto a l. W.	s. sunshine a l. W.
24	1 5	79	-	-	4 1 8	82	F. C. - -	- -	a l. C. F. -
25	1 4	78	1 9	81	1 9	80¼	a l. C. A. R.	W. and C.	W. and C.
27	1 5	77½	1 7	79	1 7	78	C. and W. A. m. R.	C. and W.	C. W. very rainy
28	1 4	78	1 8	80	1 9	80½	a l. C. A. m. R.	C. - -	F. s. sunshine
29	1 5	78½	1 9	81	1 9	81	C. and W.	C. and W.	C. and W.
30	1 8	79½	1 9	79⅓	1 10	79⅔	a l. C. -	ditto -	a l. C. s.
31	1 4	78½	1 8	82	1 8	82½	a l. C. -	s. sunshine	s. sunshine
Novem. 1	1 5	78	1 11	82½	1 9	82½	s. but C. -	a l. W. sunshine	a l. W. sunshine
3	1 8	78	1 10	83	1 10	84	s. sunshine	s. ditto	s. sunshine
4	1 7	79	-	-	1 11	83⅔	s. ditto -	- -	s. sunshine
8	1 4	79	1 9	82	1 9	79	s. a l. C. -	sunshine a l. W.	s. but C.
10	1 6	78½	-	-	1 9	84	s. sunshine	- -	s. sunshine
11	1 6	78	1 12	82	1 8	83¼	F. A. m. R. and T.	s. a l. C. -	C. and distant T.
12	1 6	77	1 9	84	-	-	sunshine a l. W.	a l. C. distant T.	
14	1 5	78	1 10	85	1 11	81	s. sunshine	sunshine W. and T.	R. C. and distant T.
15	1 4	76¼	1 9	82¼	1 10	83	s. but C. -	sunshine a l. W.	s. sunshine
17	1 5	79	1 10	83¼	1 9	81	a l. C. -	s. sunshine	C. A. R. with T.
19	1 7	78½	1 7	82	1 9	81½	s. sunshine	At 5 min. before 11 a very violent earthquake—d°	s. but C.
21	1 7	77½	-	-	1 11	82½	s. sunshine -	s. sunshine	s. sunshine
22	1 5	76	1 8	81½	1 8	82	a l. C. A. m. R.	a l. W. -	s. ditto
Decem. 5	1 4	77½	1 9	82½	1 9	82	s. and F. A. m. R.	a l. W. -	sunshine a. l. W.
10	1 6	77	1 7	80	1 8	81	F. A. m. R.	C. and rainy	a l. C. and W.

1795.	Time. 7 A.M.	Thermometer.	Time. 12.	Thermometer.	Time. 5 P.M.	Thermometer.	Various Hours.		Thermometer.	1st Observation. Weather.	2d Observation. Weather.	3d Observation. Weather.	4th Observation. Weather.
	o ,	o	o ,	o	o ,	o		o ,	o				
	E.		E.		E.			E.					
Jan. 17	1 6	77⅔	1 8	82	1 7	78	- -	-	-	C. A. R.	C. strong west wind	C. A. and T.	
19	1 6	78	1 10	84	-	-	-	-	-	s. sunshine	s. distant T.		
20	-	-	1 10	83½	-	-	-	-	-	- - -	s. sunshine		
21	1 6	77½	1 12	82½	1 9	81½	2 P.M.	1 10	82½	a l. C. s. -	s. sunshine	s. a l. C. -	distant T.
22	-	-	1 10	78¼	-	-	10 A.M.	1 8	76	- -	C. - -	- -	C. A. m. R. in the N.
23	-	-	1 10	79	1 9	79	2 P.M.	1 10	76⅔	- -	s. a l. C. -	s. a l. sunshine	C. a high north-west wind
24	1 6	75¼	1 10	81⅔	1 10	82	-	-	-	s. sunshine	s. sunshine -	s. sunshine -	
26	1 7	77	1 9	81	-	-	2 P.M.	1 10	82	a l. C. s. -	s. sunshine	- -	s. sunshine
28	1 5	74	1 8	77	1 7	75	9 A.M.	1 6	75	C. A. m. R.	C. with R. -	C. a l. R. -	C. A. m. R.
29	1 8	73	1 9	81	-	-	12 P.M.	1 8	77	s. sunshine -	a l. C. -	- -	a fine N.
30	1 7	74	1 9	81	1 9	82	11 P.M.	1 8	78	s. sunshine	s. a l. C. -	S. a l. C. -	a fine s. N.
31	-	-	1 8	82	-	-	-	-	-	- -	s. sunshine		
Febr.	8 A.M.				4 P.M.								
2	-	-	1 10	84	-	-	-	-	-	sunshine, a l. W.			
3	-	-	-	-	-	-	10 P.M.	1 7	78¾	- -	- -	- -	a fine N.
4	1 6	75⅔	1 8	79⅔	-	-	11 P.M	1 7	76½	s. a l. C. -	s. a l. C. -	- -	a s. fine N.
5	1 5	76	1 10	84	-	-	10 P.M.	1 9	82	s. a l. C.	sunshine, a l. W.	- -	a s. fine N.
6	1 6	78	1 9	84	2 1 10	84½	12 P.M	1 7	80	F. a l. W. -	a l. W. -	s. sunshine -	a s. fine N.
7	1 6	78	-	-	-	-	11 P.M.	1 7	79	a strong wind	- -	- -	a fine N.
8	1 7	79	1 11	82½	-	-	-	-	-	s. and mild -	very W.		
9	1 7	79	1 11	82	1 11	82¼	11 P.M.	1 8	78	s. sunshine	a l. W. -	a l. R. and C.	a C. N.
10	1 8	77½	1 7	82	1 9	84½	-	-	-	s. sunshine -	s. sunshine -	s. a l. C.	
11	1 7	77	1 10	84	-	-	11½ P.M.	1 9	79⅔	s. sunshine -	distant T. -	- -	a s. fine N.
12	1 6	76	1 8	82	1 11	84½	10 P.M.	1 7	79	s. - -	s. sunshine	s. sunshine, and distant T.	a C. N.
13	1 7	78	1 8	84½	1 8	79	-	-	-	s. sunshine -	distant T. -	R. and T.	
14	-	-	1 10	82½	1 10	81¼	10 P.M.	1 9	78	- -	loud T. C. -	s. and C. -	a s. C. N.
16	6 1 8	76	1 8	83	1 8	84½	7 P.M.	1 10	80	s. A. m. T.	a C. S. -	distant T. -	heavy near T. & L.
17	1 7	78	1 8	83	1 10	84½	11 P.M.	1 9	80	s. sunshine -	s. sunshine	s. sunshine -	a s. C. N.
22	-	-	-	-	-	-	2 P.M.	1 11	84½	-	- -	- -	s. sunshine
23	1 9	78	-	-	-	-	1 P.M.	1 10	84⅔	s. sunshine	- -	- -	s. ditto
26	-	-	-	-	-	-	3 P.M.	1 9	76	- -	- -	- -	a C. N.
27	1 10	77	-	-	1 11	84	-	-	-	s. sunshine -	- -	s. sunshine	
28	1 9	79	1 11	84	1 11	83½	-	-	-	s. ditto -	a l. W. -	distant T.	
Mar. 11	1 6	79	1 8	83	1 10	80	11 P.M.	1 8	77	a high wind	a high wind	a high wind -	a C. N. A. R.
12	1 6	78	1 8	80	-	-	11¼ P.M.	1 8	76½	C. and W. -	C. R. and W.	- -	a l. R. and C.
13	1 6	77	1 10	82	1 9	81	12 P.M.	1 8	78¼	a l. C. A. R. N.	s. sunshine -	s. a l. C. -	a s. N.
14	1 6	79	1 10	83	1 10	82	12 P.M.	1 8	79	s. sunshine	s. a l. C. -	s. and C. -	a R. N.
16	1 6	78	1 9	84	1 10	82	12½ P.M.	1 9	79	s. ditto -	s. sunshine -	s. and C. -	C. distant T.
17	1 6	79	1 9	84	1 10	83	11 P.M.	1 8	79	s. ditto -	s. ditto - -	s. a l. C. -	a s. C. N.

XIV. *Particulars of the Discovery of some very singular Balls of Stone, found in the Works of the* Huddersfield *Canal. By Mr.* Benjamin Outram, *Engineer to the* Huddersfield *Canal Company. Communicated by Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read April 21, 1796.

The Huddersfield canal is to be carried through that chain of mountains which extends from the Peak of Derbyshire, in a northward direction through Yorkshire, &c. into and through a great part of Scotland, by pursuing from the navigation at Huddersfield a deep and narrow valley to Marsden, where it enters the north-eastern foot of one of these mountains, called Pule Hill, under which it is to be extended south-westwardly by a subterraneous cut or tunnel to the foot of Stand Edge Hill, or Brunn Clough, where it again excavates; and pursuing the bottom of a deep valley into Saddleworth, passes along the banks of the Tame to Ashton-under-Lyne, where it joins the canals that extend to Manchester, Stockport, Peak Forest, &c.

In the latter end of the year 1794, the miners employed by the canal company began to perforate the north-eastern foot at Pule Hill; the strata they first cut through consisted of a greyish coloured shale, the beds or laminæ of which did not lie quite horizontal, but dipped or inclined a little to the westward. The strata continued regular till the workmen had

perforated about 240 yards in length from the entrance of the hill, and were about eighty yards deep from the surface of the ground immediately over them, when they discovered on the north side of their work a *fault*, *throw*, or *break* of the strata, which was filled with shale, reared on the edge, and mixed with softer earth, and in some places with small lumps of coal. In continuing to pursue the direction of the tunnel, this fault occupied by degrees more of the space of the tunnel, for about forty yards in length, when it nearly occupied the whole tunnel, which is near four yards in width; and at about five feet from its southern margin it contained a rib of limestone, near four feet thick in the bottom, but not quite so thick at the top of the tunnel; and on each side this rib it contained balls of limestone promiscuously scattered, and of various sizes, from one ounce to upwards of 100lbs. weight.

The rib and balls of limestone were first found at about 280 yards from the north-eastwardly end of the tunnel, where it is about 90 yards in perpendicular depth from the surface; and the workmen have now pursued the tunnel to near 350 yards from the entrance, and the rib of limestone and balls continue nearly the same; the rib has varied a little in thickness, and has not pursued a straight line; it in one place nearly left the tunnel to the northward, but in a few yards turned southward, to its former direction. The limestone of the rib is not perfectly pure, that in the balls is still less so, but it makes a good lime for cement. The balls when broken appear to be mixed with a kind of pyrites, in small particles, near their outward edges; their form is very peculiar, being similar in all their sizes; it is not perfectly globular, being flattened a little on two opposite sides, which appear to have been

the poles when in a revolving state; and each ball is more or less furrowed in a latitudinal direction, as if, when revolving round its axis, and taking its *fixed* from a more *fluid state*, it had met with some resisting substance.

Though the surface of this country is very rocky, it does not discover limestone any where within 20 miles of this tunnel; yet if the strata near the limestone at Buxton to the south, and at Clitheroe to the north, are examined, it will appear probable that the base of these hills is limestone at *some depth*, and this *fault* discovered in the shale probably extends from the limestone bed beneath; and the rib of limestone and balls, which, with other mixed substances, fill up this crevice or fault, were probably thrown thither from the mass beneath, by the volcanic eruption which first occasioned this *break* in the strata, or by some subsequent eruption of the same kind.

XV. *Account of the Earthquake felt in various Parts of* England, *November* 18, 1795; *with some Observations thereon. By* Edward Whitaker Gray, *M. D. F. R. S.*

Read May 12, 1796.

HAVING been desired by the President of the Royal Society to draw up, from various letters transmitted to him and to the Society, and from such other authentic information as I might be able to procure, an account of the earthquake which was felt in most of the midland counties of England, on Wednesday, November 18, 1795, I beg leave to lay before the Society the following account thereof; which, however imperfect it may be, contains all the material information I have been able to obtain upon the subject.

The earthquake happened, as is already said, on Wednesday, November 18, about eleven o'clock at night. The state of the weather, and other circumstances previous to it, are described in some of the letters hereafter noticed. Before I proceed to take notice of them, I shall endeavour to give a general idea of its extent.

It appears that the shock was felt as far to the north as Leeds, and as far to the south as Bristol. To the east it was felt as far as Norwich, and to the west as far as Liverpool. Its extent from north to south, therefore, was about 165 miles;

and its extent from east to west about 175. In this latter direction, or rather from north-east to south-west, it may be said to have reached nearly across the island.

The counties in which I have any account of the earthquake having been perceived are Somersetshire, Wiltshire, Oxfordshire, Buckinghamshire, Northamptonshire, Huntingdonshire, Norfolk, Lincolnshire, Leicestershire, Warwickshire, Gloucestershire, Herefordshire, Worcestershire, Staffordshire, Cheshire, Derbyshire, Nottinghamshire, Yorkshire, and Lancashire.

To those counties may, I think, be safely added Rutlandshire, Berkshire, Bedfordshire, Cambridgeshire, and Shropshire. I have not indeed met with any account of the earthquake from either of them; but, whoever will examine the situation of these counties, with respect to those above enumerated, will find it difficult to conceive that they were not, in some degree, affected by it."*

Perhaps a general idea of the extent of the earthquake cannot be better obtained, than by supposing the four places already mentioned as its extreme points, namely, Bristol, Liverpool, Leeds, and Norwich, to be joined by right lines, so as to form a quadrangle. That quadrangle will comprise, as accurately as such a figure can be expected to do, the parts to which it may reasonably be presumed to have extended.

Respecting the effects of the earthquake in many of the counties abovementioned, I have not been able to collect any particulars which appear to me worth relating; with regard

* I know it has been said that earthquakes have been felt at two places distant from each other, and not at an intermediate place; but I see no sufficient reason for supposing that to have been the case in the present instance.

to such counties, therefore, I think it unnecessary to do any thing more than to record its having reached them. And, for such record, I have, in some instances, considered the newspapers of the respective counties, or some such public testimony, as sufficient authority; provided the situation of the place where the earthquake was said to have been felt was such as to come within the general outline of its extent.

I shall now proceed to those details respecting the earthquake which have been received from various counties; taking them in the order in which those counties are mentioned above.

From Belton, the seat of Lord BROWNLOW, in Lincolnshire, the following account was sent by Mr. CHRISTOPHER DRIFFIELD, in a letter dated November 19.

" We had yesterday a most violent hurricane of wind, which " began about eight o'clock in the morning, and continued " till about one. After the wind settled we had a heavy rain, " which ceased about four in the afternoon, and it was a very " fine evening; but, about eleven o'clock, as near as I can say, " we felt a terrible shock of an earthquake, which lasted about " two seconds. It shook the chair in which I was sitting, be- " ginning with a violent rumbling, or noise, as if some large " carriage had run against the gate-post. It went from north " to south. It was generally felt at Grantham, and in this " neighbourhood."

The following account of its effects in Leicestershire, is given in a letter from Mr. WILLIAM KING, dated Belvoir castle, November 30.

" The earthquake happened, as near as I can collect, about " 5 or 10 minutes after eleven in the evening. The wind

" during the day had been very brisk, but towards the even-
" ing (6 o'clock) it became almost totally calm, and continued
" so the whole night. The shock was strong enough at the
" castle to waken those who were asleep. Some, though but
" few, thought it was an earthquake; the general opinion was
" that something had fallen down in the room over head; or
" had struck the floor underneath; or that an adjoining par-
" tition or door had received a blow. Pendulous bodies were
" put in motion, as well as some doors that were not shut.
" Instances of both these happened in the castle, and they,
" in some measure, confirm an opinion that the shock was un-
" dulatory. It happened at a time when few were out of doors,
" but some were, and they all agree that the shock was not
" attended with any extraordinary light; neither was there
" any ball of fire, nor any singular motion, in the air.

" The Rev. Mr. PETERS, whose residence is at Knipton, a
" mile and a half from the castle, gives the following account.
" He was going to bed, but, at the instant, stood with his
" back against a wall; he found the whole room shake, par-
" ticularly the bed curtains. As he had felt many earthquakes
" in Italy, he immediately knew it to be one, and was atten-
" tive to the consequences. He declares that the air was calm
" at the time; had no noise in it; nor did he perceive any ex-
" traordinary light. Knipton is in a low situation, and was
" shaken as much as the castle."

Of its effects at Kenilworth, in Warwickshire, the following description is given by ROBERT AUGUSTUS JOHNSON, Esq. F. R. S. in a letter dated November 25.

" The earthquake was felt at Kenilworth about twenty mi-
" nutes after eleven; but our common clocks are usually kept

" with the sun, and were therefore, at that time, nearly a " quarter of an hour too fast; by true time, I reckon it to " have been six minutes past eleven. I had lain down some " little time, when I felt the bed raised up. My first idea was " that a large dog had got under it, but the immediate shak- " ing of the curtains, and of the room, convinced me this " could not be the case. All those who were in bed, and " awake at the time, describe it in the same way; every one " supposing that their beds were raised up by some living " creature under them. It was not so universally felt by per- " sons who were up; those who were engaged in conversation " did not, in general, perceive it; but, of those whose attention " was less occupied, some felt their seats slipping from under " them, others saw things move which were hung up, and " some heard the doors and windows rattle. Many say it was " attended with a low rumbling noise, but this I did not hear. " The motion, as well as I can judge, continued nearly two " seconds. The evening was perfectly calm, after a most tem- " pestuous night and day, with the wind nearly south-west. " The barometer, for thirty-six hours preceding, had varied " very remarkably. On Tuesday morning, the 17th, it stood " at 30.23, but sunk gradually, during the course of that day, " and more rapidly in the night: to the best of my recollec- " tion, it was on Wednesday morning at 28.63, and that night, " a little before the shock was felt, at 28.8. The thermometer, " in a room without a fire, was about 48 or 49° the whole " day. I am not able to state the degree of moisture in the " air, (my hygrometer being broken,) but it was loaded with " humidity, making the passage walls and floors damp. The " effects of the earthquake were most sensibly felt on the tops

" of hills, or on high ground; but I have not heard of its " having done any damage in this country."

From Worcester the following account of it was sent by Dr. JOHNSTONE of that city, in a letter dated November 24.

" The earthquake was chiefly felt by persons in bed, about " eleven o'clock, or five minutes after, who describe the sen- " sation to have been as if some person under the bed had " heaved it up. That sensation was preceded, the instant be- " fore, by a noise which some call rumbling, and which others " compare to the falling of tiles, though none fell from the " houses where they lived. Many persons heard the windows " and doors of their rooms rattle at the same time, which in- " creased their alarm. Thunder and lightning had been ob- " served some days before; and several persons, of a delicate " state of health, passed the night of the 18th in a restless " uneasy manner, without knowing why, though very much " in the manner in which they used to be affected by thunder " and lightning."

In Derbyshire the shock appears to have been very severe. A description of its effects, not only upon the earth, but also under its surface, is contained in the two following letters from Mr. WILLIAM MILNES, of Ashover: the first is dated November 20.

" On Wednesday night, about a quarter past eleven o'clock, " a severe shock of an earthquake was felt here. I felt it very " sensibly; at first I heard a rumbling kind of noise, and im- " mediately after it appeared as if some person or persons had " violently forced into the room; the bed, and every thing " else, shaking very much. The workmen in Gregory mine

" were so much alarmed by the noise, and the sudden gust of " wind that attended it, as to leave their work; some expect- " ing that the whole mass of *bunnings* above them, which " contains many hundred tons weight of rubbish, had given " way, and that they should be buried in the ruins; others, " who were at work near the new shaft, supposed that the " curb which supports the walling had given way, and the " whole shaft had run in. Several chimnies were thrown " down, and several families left their habitations; indeed " such a general alarm was never known in this neighbour- " hood."

The gust of wind mentioned by Mr. MILNES being considered as a remarkable circumstance, he was desired to make some farther inquiry concerning it: in consequence of which a second letter was received from him, dated December 4, as follows.

" I have examined all our miners separately, and, from the " following circumstances, I think there cannot be a doubt " but the wind which was felt in the mines, on the 18th of last " month, rushed into the shafts from the surface.

" Those men who were at work in the pumps, which are " a considerable depth below the waggon gates, and have " no communication therewith, did not feel the wind; but " heard, in the first place, a rushing rumbling kind of noise, " which appeared to be at a distance, and to come nearer and " nearer, until it seemed to pass over them, and die away. " Those who were in the waggon gate which has a commu- " nication with the engine shaft, and the new shaft, felt a " very strong current of wind; which, one man says, conti- " nued whilst he walked about six or seven yards, and came

" along the gate, as if it came from the new shaft; he had no " light, but, as he went along the gate, the sides thereof, " where he laid his hands, felt as if they were going to close " in upon him.

" The only one who saw any appearance of light, on that " evening, in this neighbourhood, (that I have been able to " make out,) was a person who lives with Mr. ENOCH STE- " VENSON, the miller, at Mill Town. He informs me that, as " he and another man were returning from Tideswell, he saw, " when he got upon a piece of high land near Moor-hall, on " the road to Chatsworth, an uncommon light; and, when " looking towards Chesterfield, the sky appeared to be open " for about the length of a mile, the colour pale red, and con- " tinued so while he awakened his fellow servant, who was " asleep in the waggon, to shew him (as he described it) the " strangest flash of lightning that ever was seen. From his de- " scription, the range of it was from east to west; and so low " in the horizon that, had he not been upon high ground, he " could not have seen it."

From Wirksworth, in the same county, (Derbyshire,) two letters respecting the earthquake were received from the Rev. ABRAHAM BENNET, F. R. S. of which here follow extracts. The first letter is dated November 19.

" About twenty minutes past eleven o'clock last night, the " shock of an earthquake was perceived in this town, and, as " I have been informed, at Derby, Ashover, Bakewell, Win- " ster, and other neighbouring villages. I happened to be in " bed at the time, and was awakened by a noise, which I first " supposed to be like the roaring of a chimney on fire, then " thought it was a carriage in the street, and should not have

" suspected an earthquake; but this morning I was told that " many persons had been much alarmed, said they heard a " noise resembling the falling of a chimney, and went out to " examine whether that was the case. A person just arrived " from Derby says, that a ball of fire was seen to pass over " the town, at the time the noise was heard. Many said their " beds shook, and that they felt something like an electrical " shock. At Derby some bricks were thrown down from " chimnies. The noise was also described as resembling the " drawing of a table over a floor, with a louder explosion at " the last.

" The wind had blown violently from the south-west all " day, but became calm in the evening, and the sky cleared. " At the time the earthquake happened it was remarked that " the air became very cold; and this morning the ground was " covered with snow; which now melts, and the wind is " changed to the north-east.

" These circumstances seem to favour the supposition of " earthquakes being caused by electricity; but it is only from " a collection of numerous facts, that any rational theory can " be formed on the subject."

In his second letter, dated December 21, Mr. BENNET says; " I have been at Derby, and made inquiry about the ball of " fire said to have been seen there, but could obtain no ac- " count of any thing more than that several persons, who hap- " pened to be out, perceived a flash of light, which they de- " scribed as being like an opening of the sky. I had written " to Mr. CHATTERTON to make every inquiry he could, but he " heard nothing more than this, besides what was observed by " others. I wrote also to Mr. WATSON, of Bakewell, who says

" he had been in bed about a quarter of an hour, when he was " astonished by a hushing rumbling noise, and was immedi- " ately shook in bed, by a lifting up, of first his head, and " then his feet, three times in about three seconds; his bed " standing south and north, he thought it came from the south. " At an inn there the servants were frightened by the glasses " shaking upon a table; some gentlemen, in another room, " felt nothing of it. One person at Bakewell observed a flash " of light like lightning; and Mr. BUXTON's house, (at Bake- " well,) which stands upon a limestone rock, was shook till " his bell rung.

" The Rev. Mr. PEACH, of Edensor, was shook in bed, and " heard a noise in his room, like the collision of two stones.

" At Chesterfield some chimnies fell."

The county of Nottingham, however, appears to have been that in which the earthquake was most severely felt. From that county two very circumstantial accounts of it have been received; one of them from the Rev. EDWARD GREGORY, in a letter dated Langar, December 12, as follows.

" I was, on the 18th of November, at Wollaton, (the seat " of Lord MIDDLETON,) about three miles to the west of Not- " tingham, where I felt the shock of the earthquake. A vio- " lent gale of wind, which blew from the south-west, raged " with uncommon fury all the morning, and brought on heavy " rain about noon; the storm still continuing with unabated " violence. About three o'clock the wind changed to north- " west, and the tempest presently subsided. The clouds now " separated, and formed themselves, on the northern quarters of " the horizon, into those very large white mountainous clouds " which, in the summer months, generally precede a thunder

" storm. About sunset these clouds were very much dis-
" persed; the air became clear, felt sharp and elastic, and
" every appearance of a frost came on in the northern parts of
" the sky. When the company assembled in the south draw-
" ing-room, previous to dinner, my attention was much struck
" with the aspect of the sky in the south and south-east quar-
" ter; in this direction, a cloud, very black and lowering, ex-
" tended itself over this part of the hemisphere. The margin
" of the cloud, which was nearly parallel to the horizon, was
" fringed, to the extent of at least forty degrees, from the south
" towards the east, and to the breadth of perhaps a degree and
" a half, with a very bright white light, which had very much
" the appearance of white satin. This light was shaded, to its
" whole extent, as it were with a veil of a deep muddy purple
" colour. The white light, seen below this gloomy purple
" haze, and farther contrasted by the very dark surface of so
" extensive and lowering a cloud, formed a very striking ap-
" pearance; so much so as to induce me to call Lord MID-
" DLETON, and others of the company, to the window, to look
" at it, and to remark to them, that the very angry and trou-
" bled aspect of the sky led me to apprehend we should have
" more of the tempest in the course of the night. I will here
" remark that it was now near five o'clock; the sun was gone
" down too low to cast any considerable degree of light on
" the clouds near the western horizon, much less could he il-
" luminate any cloud so low in the south-east as this was with
" so bright a light. The moon, indeed, was near the western
" extremity of this cloud; but its light, even had it been a
" full moon, (whereas it yet wanted more than twenty-four
" hours of being in the first quarter,) was far too weak to cast

" on the margin of the cloud so intense a light, and that to
" so great an extent along it. For these reasons, I was fully
" persuaded that this luminous appearance was occasioned by
" electric light, with which I concluded the cloud to be highly
" charged. We went down stairs to dine, and returned again
" to this room about eight o'clock, to pass the evening; I then
" looked again at the sky; every extraordinary appearance
" had now vanished, the night was dark and gloomy, the air
" quite calm and mild. At between twenty and twenty-five
" minutes after eleven o'clock, we were all extremely sur-
" prised and alarmed at a sudden blast, (so I should term it,
" rather than explosion, because it had not that sharp, com-
" pressed elastic tone I annex to the idea of an explosion,)
" which burst out instantaneously, somewhat below the ze-
" nith, to the west; and, as I conjectured from the direction
" in which the sound was heard, seemed to rush through the
" air towards the east with great velocity, and to meet with
" considerable resistance to its motion; for it made a whiz-
" zing noise as it passed over us. At the instant the blast
" burst out, it was accompanied with a very loud, deep-toned,
" hollow, sullen sound, not altogether unlike a deep groan.
" We were all amazed at this hideous noise; some thought
" the window of the great room (that in the centre tower over
" the hall) had fallen in: every one of us thought some bad
" accident must have happened to some part of the building.
" While we were all forming our various conjectures, I, who
" sat close to the wall, (a north wall,) leaning with my arm
" and shoulder on the surbase, felt myself shoved from it, and
" my chair shaken under me, with a very quick tremulous
" motion. All the company (eight or ten) felt the same

" sensation from the shock; which was so forcible, that some
" thought I had fallen from my chair on the floor, and had oc-
" casioned the concussion they felt; when they saw this was
" not the case, they then imagined the servants in the dining
" room below us, in removing the tables, had thrown down
" one of them. When I felt the wall shake, I had no doubt it
" was an earthquake, and I told the company so; but that it
" was now over, and we were all safe, and had no reason to be
" farther alarmed. I had scarcely uttered these words, when
" we were shaken again. This second shock was more undu-
" latory than the first; at this time I was standing, and felt
" myself lifted up a little, as the shock passed along. We were
" all now under great apprehensions, lest other and more vio-
" lent shocks might succeed those we had already felt; but
" after a few minutes had elapsed, without our perceiving any
" thing more, our alarm subsided. Such of the company as
" had withdrawn returned to us, to inquire what could be the
" cause of the strange noise, and the shocks which succeeded
" it; not being yet aware that it was an earthquake. I have
" already mentioned the time the blast was heard; the first
" shock of the earthquake came on, as near as I can guess,
" about a minute after it; and the second shock succeeded the
" first at the interval of twenty seconds. I went down to the
" steward's room, and servants' hall, to inquire what was felt
" of the earthquake, and whether they saw any light at the
" time they heard the blast. They said they saw no light
" whatever, but that the shock caused the glasses to dance
" upon the table, in such a manner that they imagined some-
" body was drumming under the table with their knees. I
" then went into the garden, to look at the sky; it continued

" much as it was at eight o'clock, dark and gloomy, without
" any particular appearance; the air was quite calm, and mild.
" When I got up in the morning, I was surprised to see the
" country covered with snow; the trees in the park were
" loaded with it; I believe no morning in the last winter
" could have presented more appearance of extreme severe
" weather. My man slept at Nottingham; at the house where
" he was, the earthquake was so violent as to cause the cham-
" ber bells to ring. I inquired of two soldiers belonging to
" the barracks, situated on the high ground in the park near
" that town, what was felt of the earthquake there. They
" told me the shocks were very strong, so much so as to alarm
" them greatly, and to shake the plates off the shelves. I
" farther inquired whether any blaze of light, or ball of fire,
" was seen when they first heard the blast, or during the
" shocks of the earthquake. They said, that no light was seen
" at either of these periods, nor did the centinel on guard per-
" ceive any singular appearance whatever.

" Here, in the vale of Belvoir, most people were in bed, and
" were awakened by the shocks, which they describe to me, as
" raising up the bed, and then shaking it; so that at first they
" thought somebody had hid themselves under the bed, and
" was playing tricks to frighten them; but, when they heard
" the doors rattle violently, the plates on the shelves move,
" glass, china, &c. jar together, they were aware that the cause
" of all this was an earthquake. Very few were awakened by
" the blast, and none I have conversed with saw any meteor,
" or other appearance of light. The shock was so strong at
" Colston Basset (two miles from Langar) as to shake a small
" dog off the bed on which it slept. Bricks, such as were

" most likely ready to fall by the first gust of wind, were shaken
" off the chimnies. Some small part of the bank of the canal
" near Redmile was thrown back again into the canal; which
" is supposed to have been done by the earthquake, the bank
" having been in an uninjured state the preceding evening.
" Concerning the direction of the shocks, people here do not
" accord; this is not to be wondered at, as most of them were
" awakened out of their sleep, and could not collect their
" thoughts together soon enough to ascertain this circum-
" stance. In general, those who were awake, previous to the
" shocks, seem confident they came from the north-east, but
" many think they came from the south. The first shock felt
" to me so tremulous that I could not form any judgment con-
" cerning its direction; my chair was shaken with a kind of
" vertiginous motion. The second shock seemed to come
" from the north, perhaps a few points to the west of it.

" When I heard the blast burst out, and rush on, with a
" whizzing noise, through the air, I immediately concluded
" that some electric meteor (which, I supposed, took fire
" at that time) was the cause which produced that alarming
" dismal sound which we heard; and that the concussion in
" the atmosphere, being communicated to the earth, had
" shaken it with such forcible agitations as to cause the
" shocks of the earthquake. The clouds on the preceding
" evening, being to all appearance very highly charged with
" electric fire, confirmed me in this opinion; yet, as far as
" I can ascertain, and I have diligently inquired of various
" persons who were out, and in situations where they had
" a considerable extent of view, no meteor, nor light what-
" ever, was seen hereabouts at any time that night. Those

" in bed (as I have already related) felt themselves lifted up,
" and shaken. From this circumstance, one is led to imagine
" the cause of the earthquake to have been within the earth;
" yet, all the circumstances considered, I incline to think some
" violent concussion in the air occasioned it. The blast
" (whatever was the cause of it) was undoubtedly in the
" air. The very heavy fall of snow during the night seems a
" singular circumstance; it might be accidental, yet I feel a
" bias to think it so far connected with the cause of the earth-
" quake, as to have been the result of a sudden and extraor-
" dinary change in the atmosphere, brought about by some
" electric agency; and that it was a collateral effect of the
" cause (be that cause what it may) which occasioned the
" earthquake.

" I have been, since the earthquake, on the eastern side of
" Derbyshire; it was felt there very smartly. I remark that
" most places in high situations were shaken with more vio-
" lence than those in flat countries; for instance, in Derby-
" shire; at the barracks near Nottingham; and in the villages
" which, in the south direction, bound this vale."

The other account from Nottinghamshire is given by Dr. STORER, in the following letter, dated Nottingham, March 12, 1796.

" On the 18th of November last, at eleven o'clock at night, a
" very smart shock of an earthquake was felt in this town and
" neighbourhood. It was preceded by a noise, which appeared
" to me like that of dancing in the room over that in which
" I sat, accompanied by a clattering of the furniture; and
" beginning, as seemed to me, at the north-east, and passing
" to the south-west corner of the room. To others the reverse

" of this appeared to be the direction; but all who attended
" to the circumstance of direction, attribute to it a southerly
" and northerly course; and every person, whether within or
" out of doors, referred the noise to something above their
" own situation.. I think the noise continued about three
" seconds, and was instantly succeeded by the shock; which
" was so considerable and alarming as to give me the idea of
" an earthquake, from the moment of its commencement, and
" to make me imagine that I saw, as well as felt, an elevation
" of the hearth, on which my feet rested as I sat; but which
" was not found to be displaced, in respect to surrounding ob-
" jects. That the walls of the room suffered a considerable con-
" cussion was evident, from the vibration of the window-shut-
" ters, pictures, and every other pendulous object in the room.

" I had guessed the duration of the shock itself to be four
" seconds; but a friend of mine (who had experienced similar
" shocks in Carolina) assured me that, after the first impulse,
" he had time to look at his watch, and to count four seconds
" before its termination: in his opinion, the duration of the
" shock could not be less than five seconds and a half. Such
" was the violence of it, that (though no material damage hap-
" pened to the buildings) most people in this town and neigh-
" bourhood, who were asleep, were awaked; and many so
" suddenly and completely as to be conscious of having expe-
" rienced an earthquake.

" Nothing connected with this object has surprised me so
" much as the extreme disparity of the sensations communi-
" cated by the shock to different people, in the same room,
" and in all respects similarly circumstanced. Every person

" equally heard the noise, and had their attention awakened " by it; yet some felt the shock in an alarming degree, while " others, at the distance of a few yards, perceived nothing but " the noise, and the rattling of the furniture; a few felt some- " thing like an electrical shock, and nothing else.

" Though there are many mill-ponds, canal reservoirs, and " other considerable pieces of water, in this county, which " would have been liable to untoward accidents from any ma- " terial agitation or elevation of their surfaces, I do not find " that any such thing was observed.

" As it is unusual for workmen to be in the coal mines at " that hour of the night, I have heard of but one instance " where that was the case. In one of the mines there were a " man and a boy; the former felt nothing; the boy, who was " nearer the shaft, perceived a rumbling, which he supposed " to be at the top of the shaft, but felt no shock.

" I have seen few people here who were in the streets, or " without doors, at the moment the earthquake happened, " and of these no one perceived any light in the heavens; but " it is reported that a light was seen by the passengers in " some of the coaches; and a very intelligent gentleman of " Derby told me that, being in the street, he perceived, at " the instant of the concussion, a remarkable coruscation, " proceeding from the south-west quarter of the heavens, " (which he could not then see,) and producing a gleam simi- " lar to a distant flash of lightning, but of longer continuance. " Others, at Derby, saw the same thing through their win- " dows.

" In every direction, to the distance of 25 miles at least

" from this place, the shock was felt with equal force; be-
" yond that distance, my information is too inaccurate to be
" stated here.

" The state of the atmosphere that accompanied this phæ-
" nomenon is scarcely less remarkable than the earthquake
" itself. In the night of the 17th it had blown with some vio-
" lence from the south-west; in the morning the gale increased,
" and at eleven o'clock blew a tempest, accompanied with very
" dark dense clouds, and with a greater degree of warmth, or
" rather sultriness, than I ever recollect to have felt in Novem-
" ber, when there was no sunshine. About mid-day there fell a
" heavy rain, for an hour; after which the wind abated, the
" clouds dispersed, and at six o'clock it was a serene calm even-
" ing. At the moment of the earthquake it was perfectly still,
" and continued so at one o'clock in the morning, with the
" same degree of warmth that had prevailed in the day. At
" eight o'clock the following morning it froze intensely, and
" the ground was covered with snow.

" It being very generally agreed to refer the most formidable
" earthquakes to subterraneous causes, it may be thought un-
" philosophical to search for causes of a different order from
" those that are known to produce similar effects; yet it must
" be admitted, that many circumstances in the preceding ac-
" count conspire to connect the concussion with the very sin-
" gular state of the atmosphere accompanying it, and irre-
" sistibly to direct our inquiries to those sudden revolutions
" to which so vast a mass of elastic and heterogeneous fluid is
" liable."

Having now laid before the Society all the circumstances observed in the late earthquake that appear to me to be wor-

thy of their attention, I shall proceed to make a few observations upon them; and particularly to examine how far it is possible, by tracing the direction of the earthquake, to determine whether it was produced by a central force acting in all directions, or whether, like a blast of wind, it had a progressive motion one way only.

And first I must observe, that all ideas of its direction, founded on the sensations of those who felt it, are so vague and contradictory as to render it impossible to draw any conclusion from them. Mr. GREGORY, who in his letter says the blast came from the west, says also, that people in general seem confident that the shocks came from the north-east, but that many think they came from the south; and that, to himself, the second shock appeared to come from the north. Dr. STORER says the direction appeared to him to be from the north-east to the south-west, but to others it appeared the reverse. The accounts of the earthquake's direction in the county newspapers are as various as the above; but I think it needless to give any farther proofs of what I have advanced, and shall only observe that, various as the opinions of those who felt the earthquake were, with respect to its direction, the greater number of persons agreed in thinking it to have been from some northern point towards a southern one; which, as we shall presently see, is as contrary as possible to that direction which is deduced from observations of the time at which it was felt in different places.

In the county papers, the earthquake is said to have been felt at Bristol, and in some parts of Gloucestershire, some minutes before eleven. At Worcester, it is said (by Dr. JOHNSTONE) to have been felt about eleven, or five minutes after.

At Kenilworth, in Warwickshire, (Mr. JOHNSON,) six minutes after eleven. At Ashover, in Derbyshire, (Mr. MILNES,) about a quarter past eleven. At Wirksworth, in the same county, (Mr. BENNET,) about twenty minutes past eleven. And at Wollaton, in Nottinghamshire, (Mr. GREGORY,) between twenty and twenty-five minutes past eleven.* From this, about five minutes and a half are to be deducted, for the difference of longitude between Bristol and Wollaton. Great allowance must likewise be made for the uncertainties which attend observations of this kind, from the different manner of keeping clocks, and from other circumstances too obvious to be mentioned; but it must be remembered, that those circumstances are as likely to occasion error on one side as on the other; and, when the whole is fairly considered, it seems to me impossible not to feel inclined to think that the earthquake was felt considerably later in the north-east than in the south-west; in other words, that it moved progressively from the south-west to the north-east, or nearly so.

Supposing, however, that some of the abovementioned observations of time are of too uncertain a nature to admit any inference to be drawn from them, others among them are of a very different kind. Mr. JOHNSON, whose accuracy may be safely relied on, appears to have remarked the time of the earthquake with great precision, and he states it to have been, at Kenilworth, at six minutes past eleven. Mr. GREGORY, to whose exactness we may equally trust, says the blast was heard, at Wollaton, between twenty and twenty-five minutes

* I have omitted mentioning the time expressed in one or two of the foregoing letters, because it appears not to have been observed with much attention.

past eleven, and the earthquake came on about a minute after the blast. Now, if we suppose it to have been only twenty-one minutes past eleven when the blast was heard, it will bring the time of the earthquake to twenty-two minutes past eleven; and, if we allow a minute and a half for the difference of longitude between Kenilworth and Wollaton, there will still remain an interval of fourteen minutes and a half for its progress from the first mentioned place to the latter. The distance between these two places is about forty-five miles, and the situation of Wollaton, with respect to Kenilworth, is about north north-east: consequently, the observations of Mr. JOHNSON and Mr. GREGORY are, of themselves, sufficient to render it probable (as far as observations of time made in two places only can do so) that the direction of the earthquake was not very different from that above stated; at least, that it was from some point to the westward of south, towards some point to the eastward of north; which, as was before observed, is very contrary to the idea which most persons who felt it formed of its progress.

These two observations also, in my opinion, furnish another argument that the earthquake moved progressively in one direction only; for, if it had been produced by a central force acting in all directions, we should surely have expected that the effects of that force would have been most powerful where they were first felt; whereas, we have seen that the earthquake, though undoubtedly more severe in Nottinghamshire than in Warwickshire, was felt much sooner in the last mentioned county.

Of the various earthquakes felt in England within this century, those to which the one here treated of has most analogy

are, that of September 30, 1750;* that of September 14, 1777;† and that of February 25, 1792.‡ The earthquake of last year was of much greater extent than either of the others, consequently a much greater number of counties came within its influence; but there is, to a certain degree, a general analogy in the tract of country affected. It is also observable, that the direction assigned to the abovementioned three former earthquakes, is nearly the same as that I have supposed to have been the direction of the one here described.§ This recurrence of earthquakes, in former tracts, has been long observed in all countries much subject to them; and has, with great reason, been considered as a strong argument in support of the opinion, that their cause is situated within the earth.

It has also been observed in many earthquakes, as in this, that, whatever was the state of the wind some time before, it was calm at the instant the earthquake happened. This has indeed been so generally the case, as to have induced some to suppose that wind would prevent a certain accumulation in the atmosphere, which, according to their theory, is necessary for the production of an earthquake.|| Unfortunately for that theory, however, there are more instances than one of earthquakes having happened during a gale of wind.¶

* Described in the Philosophical Transactions, Vol. XLVI. page 701, *et seq.*

† Described in the Phil. Trans. Vol. LXVIII. page 221.

‡ Described in the Phil. Trans. for the year 1792, page 283.

§ See Phil. Trans. Vol. XLVI. page 722; Vol. LXVIII. page 227; and that for the year 1792, page 287.

|| See Phil. Trans. Vol. XLVI. page 676.

¶ Dr. Thomas Heberden, in his account of an earthquake felt in the island of Madeira, March 31, 1761, says, " though it has been remarked that a calm always

Some persons thought this earthquake was most severely felt in high situations; others remarked that some low places were just as much affected: upon the whole, there does not seem room for any material inference on this head.

That the waters about Nottingham should not, according to Dr. STORER's observation, have suffered any remarkable agitation or elevation, may be thought surprising; especially when it is recollected that the earthquake of November 1, 1755, which was so fatal to the city of Lisbon, occasioned an unusual agitation of the waters in various parts of this kingdom, without occasioning any perceptible motion of the earth in those parts.* A similar circumstance, however, is recorded by M. BERTRAND, respecting an earthquake which was very severe in many parts of Switzerland, on December 9, 1755, but which, as he says, produced little or no agitation of the lakes there; whereas a very considerable one had been produced in them by the earthquake of November 1, in the same year, though it was but slightly felt upon the earth.†

" attends an earthquake, no such thing happened now; a fine gale of wind blowing " before and after, as well as during the time of, the shock." (See Phil. Trans. Vol. LII. page 156.) M. BERTRAND also, speaking of an earthquake felt at Zuric, December 9, 1755, says, " Le tremblement étoit accompagné d'un vent violent, que quelques " personnes ont apperçu dès le commencement, d'autres à la fin des ébranlemens." See *Recueil de divers Traités sur l'Histoire Naturelle de la Terre, par* M. E. BERTRAND, page 289.

* See Phil. Trans. Vol. XLIX. page 351, *et seq.*

† Of so remarkable a circumstance, it may not be amiss to give M. BERTRAND'S account in his own words: " A Vevey, les rues le long du lac ont été plus agitées; les " cloches ont donné du son; quelques vases ont été renversés; des portes ont été " ouvertes; des tuiles sont tombées des toits, &c.—Ni à Vevey, ni ailleurs, sur les " bords du lac Leman, on n'a apperçu aucune hausse de ses eaux. Il est bien re- " marquable que les lacs de la Suisse aient été plus émus du tremblement de terre du

The wind felt in the mine at Ashover, was probably the effect of the blast mentioned by Mr. GREGORY. Similar blasts have been taken notice of in many other earthquakes; a rushing wind is said to have accompanied that felt in England, September 14, 1777.*

With respect to the other unusual atmospherical circumstances with which this earthquake was accompanied, I shall only observe, that similar ones, particularly black dense clouds, coruscations in the air, &c. have been noticed in several other earthquakes; yet, upon the whole, perhaps few have been attended with more remarkable circumstances of that kind. How far they were connected with the earthquake, it is impossible to determine; be that as it may, it must be allowed that they appeared to be so, to such a degree as very naturally to incline those who noticed them to be of opinion, that the cause of the earthquake was situated in the atmosphere. Upon this head I cannot help remarking, that those who entertain that opinion respecting the cause of earthquakes, seem always to conclude that the electric fluid must, in that case, necessarily be the agent. This, I think, is going too far: we surely do not know enough of the nature of the atmosphere to warrant us in making such a conclusion; and what we do know of electricity (either natural or artificial) rather leads us to conceive that the electric fluid is formidable only when concentrated, or collected within a certain space, and moving with

" premier Novembre, que de celui du neuvième Decembre; quoique le premier ne se " soit fait sentir dans les terres que légèrement, et dans un petit nombre d'endroits; " au lieu que le dernier a secoué tout le terrein, sans emouvoir les eaux. Pourquoi " cette différence dans les effets?" BERTRAND, *Recueil de divers Traités, &c.* page 283.

* See Phil. Trans. Vol. LXVIII. p. 227.

infinite velocity; consequently its effects are limited in their extent, and it is instantaneous in its operation. Whereas earthquakes have, more than once, extended their effects over immense portions of the globe; and they appear to move (sometimes at least) with a degree of slowness of which we cannot suppose the electric fluid capable, without supposing it thereby so far divested of its destructive power, as to be incapable of producing what is truly considered as the most dreadful of all natural phænomena.*

The foregoing reflections are offered as naturally arising from the consideration of the account here given, and by no means as taking either side of the great question, whether earthquakes are to be considered as terrestrial or as aerial phænomena. A question which appears to me to be involved in the greatest obscurity; and this obscurity seems to arise, not so much from a want of arguments on either side, as from the arguments on both sides being so many, and so strong, that the mind hesitates less which side to choose, than which to reject. For as, on the one hand, there are many circumstances attending on earthquakes, (particularly their frequent recurrence, in many parts of the world, in the same tract,) on which it is impossible to reflect, and not feel disposed to believe their cause situated in the earth; so, on the other hand, it seems equally impossible to reflect on the unusual atmo-

* Of the slowness here spoken of, the earthquake described in the foregoing pages affords sufficient example, particularly in its progress from Kenilworth to Wollaton; (see page 374;) but, in the account of an earthquake felt near Oxford, June 19, 1665, it is said, that "Dr. HOLDER, F.R.S. who was then at Blechington, took notice that it "was observed by those in the further part of the garden, some very discernible time "before it was observed by those in the house, creeping forward from the one place to "the other." See Phil. Trans. Vol. I. page 169.

spherical appearances, with which the earthquake here described and many others have been accompanied, and suppose all those appearances merely accidental, and unconnected with the earthquakes. It is not, however, my intention to enter into a minute examination of this part of the subject; but I cannot refrain from hinting, that those who may hereafter be inclined to do so would perhaps do right to begin by inquiring, whether all commotions of the earth which go under the name of earthquakes are really produced by the same cause.* After that, as a subject so full of difficulties ought to be viewed in every possible light, it might not be amiss to consider whether, supposing the cause of an earthquake situated in the earth, that cause may not be capable of affecting the atmosphere, in such a manner as to occasion those appearances in it which we find so difficult to explain; or whether, if the cause be supposed to be seated in the atmosphere, certain tracts of country may not (from some circumstances with which we are unacquainted) possess a peculiar disposition to bring that cause into action, by co-operating with it.

* To prove that this is not an unnecessary inquiry, it might perhaps be sufficient to refer to what has been said respecting the agitation of the waters; (page 376;) but, as a farther proof that earthquakes either differ from each other, or appear to do so to different persons, I may add, that though I have endeavoured to show that the slowness with which earthquakes sometimes move is a strong argument against their being caused by electricity, yet Dr. STUKELEY, the first promulgator of the electric theory, as one of his arguments in its favour, says that, as far as he could learn, the earthquake of September 30, 1750, was felt over its whole extent precisely at the same instant. (See Phil. Trans. Vol. XLVI. page 738.) BECCARIA also, whose theory is an electric one, (though very different from Dr. STUKELEY's, inasmuch as he considers the electric fluid within the earth to be the chief agent,) lays great stress, in support of his theory, upon the velocity with which, as he says, earthquakes move. See *Beccaria, dell' Elettricismo artificiale e naturale*. Cap. vii. § 679.

I have ventured to propose the consideration of these mixed causes,* if I may so term them, because I think it very certain that none of the theories hitherto formed respecting earthquakes, are by any means adequate to explain their various phænomena. I leave it to future theorists, should any adopt the principle, to determine which of the elements is concerned in a primary, and which in a secondary way; also what share in the attendant phænomena is to be assigned to each element.

In whatever light the subject is viewed it seems to present so many difficulties, that perhaps the best we can do is, to consider our knowledge respecting earthquakes as consisting merely in a certain number of facts, to which many more must probably be added, before we shall be able to understand the cause of a phænomenon whose dreadful effects have, in all ages, been too well known in every quarter of the world. So frequent indeed are earthquakes in some parts, that it is impossible, without being fully sensible of our happiness, to reflect

* Dr. HALES's theory of earthquakes was founded upon a sort of mixed cause: he supposed sulphureous vapours to arise from the earth, and to form clouds, the explosive lightning of which kindled the ascending vapours in the earth, and thereby caused what he calls an *earth lightning*; which lightning he considered as the immediate cause of earthquakes. Yet, so imperfect was the aerial chemistry of those days, that the experiment by which he illustrates the operation of his sulphureous vapours, consists only in mixing together nitrous gas and common air, to show the red fumes, and diminution of bulk produced. (See Phil. Trans. Vol. XLVI. page 672 and 677.) If Dr. HALES had been acquainted with modern chemistry, he would undoubtedly, I think, have made inflammable gas the basis of his theory; and indeed, when it is considered that there are great quantities of this gas in the earth, that it readily ascends into the atmosphere, and that, when inflamed, it is capable of producing the most violent effects, it appears to me that it has at least as much claim as the electric fluid, to be reckoned among the probable agents in earthquakes.

on their comparative infrequency here; which is such as to give us every reason to hope, that (whatever philosophy might gain by the extension of our knowledge on so interesting a subject) the island we inhabit will contribute but a small share of those materials which are still wanting to form a complete theory of earthquakes.

XVI. Newton's *Binomial Theorem legally demonstrated by Algebra.* *By the Rev.* William Sewell, *A. M.* *Communicated by Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read May 12, 1796.

LET m and n be any whole positive numbers; and $\overline{1+x}^{\frac{m}{n}}$ a binomial to be expanded into a series, as $1 + Ax + Bx^2 + Cx^3 +$, &c. where A, B, C, D, &c. are the coefficients to be determined.

Assume $v^m = \overline{1+x}^{\frac{m}{n}} = 1 + Ax + Bx^2 + Cx^3 + Dx^4 +$, &c.

And $z^m = \overline{1+y}^{\frac{m}{n}} = 1 + Ay + By^2 + Cy^3 + Dy^4 +$, &c.

Then will $v^n = 1 + x$, and $z^n = 1 + y$ $\therefore v^n - z^n = x - y$.

And $v^m - z^m = A \times \overline{x-y} + B \times \overline{x^2-y^2} + C \times \overline{x^3-y^3} + D \times \overline{x^4-y^4} +$, &c.

Consequently $\frac{v^m - z^m}{v^n - z^n} = A + B \times \overline{x+y} + C \times \overline{x^2+xy+y^2} + D \times \overline{x^3+x^2y+xy^2+y^3} +$, &c. Now $v^m - z^m = \overline{v-z} \times : v^{m-1} + v^{m-2}z + v^{m-3}z^2 +$, &c.....$z^{m-1}$. Also $v^n - z^n = \overline{v-z} \times : v^{n-1} + v^{n-2}z + v^{n-3}z^2 +$, &c....$z^{n-1}$. Therefore $\frac{v^m - z^m}{v^n - z^n}$ reduces to, and becomes $= \frac{v^{m-1} + v^{m-2}z + v^{m-3}z^2 +, \&c....z^{m-1}}{v^{n-1} + v^{n-2}z + v^{n-3}z^2 +, \&c....z^{n-1}}$ $= A + B \times \overline{x+y} + C \times \overline{x^2+xy+y^2} + D \times \overline{x^3+x^2y+xy^2+y^3} +$, &c.

The law is manifest; and it is likewise evident that the

numerator and denominator of the fraction, respectively terminate in m and n terms. Suppose then $x = y$; then will $v = z$; and our equation will become $\frac{mv^{m-1}}{nv^{n-1}}$, or $\frac{mv^{m-n}}{n} = A + 2Bx + 3Cx^2 + 4Dx^3 +$, &c.

But $v^n = 1 + x$, therefore by multiplying we have $\frac{mv^m}{n} = A + \overline{A + 2B}x + \overline{2B + 3C}x^2 + \overline{3C + 4D}x^3 +$, &c. Or $v^m = \overline{1 + x}^{\frac{m}{n}} = \frac{nA}{m} + \frac{nA + 2nB}{m}x + \frac{2nB + 3nC}{m}x^2 + \frac{3nC + 4nD}{m}x^3 +$, &c.

Compare this with the assumed series, to which it is similar and equal, and it will be

$$\frac{nA}{m} = 1$$

$$\frac{nA + 2nB}{m} = A$$

$$\frac{2nB + 3nC}{m} = B,$$

&c. =, &c.

$\therefore A = \frac{m}{n}$; $B = \frac{\overline{m - n}A}{1.2.n}$; $C = \frac{\overline{m - 2n}B}{1.2.3.n}$; &c.

Therefore $\overline{1 + x}^{\frac{m}{n}} = 1 + \frac{m}{n}x + \frac{m \times \overline{m - n}}{1.2.n^2}x^2 + \frac{m \times \overline{m - n} \times \overline{m - 2n}}{1.2.3.n^3}x^3 +$, &c. the law is manifest, and agrees with the common form derived from other principles.

Sch. In the above investigation, it is obvious that unless m be a positive whole number, the numerator abovementioned does not terminate: it still remains, therefore, to shew how to derive the series when m is a negative whole number. In this case, the expression $(v^m - z^m)$ assumes this form, $\frac{1}{v^m} - \frac{1}{z^m}$, or its equal $\frac{z^m - v^m}{v^m z^m}$, which divided by $v^n - z^n$, as before, gives $\frac{1}{v^m z^m} \times \frac{z^m - v^m}{v^n - z^n} = \frac{1}{v^m z^m} \times \frac{-\overline{v - z} \times : v^{m-1} + v^{m-2}z + v^{m-3}z^2}{\overline{v - } \times : v^{n-1} + v^{n-2}z + v^{n-3}z^2 +}$, &c. =

$\frac{-1}{v^m z^n} \times \frac{v^{m-1}+v^{m-2}z+v^{m-3}z^2+, \&c.}{v^{n-1}+v^{n-2}z+v^{n-3}z^2+, \&c.} =$ (when $v=z$) $-\frac{mv^{m-1}}{v^{2m} \times nv^{n-1}}$ $= \frac{-mv^{-m-n}}{n}$, which is the same as the expression $\left(\frac{mv^{m-n}}{n}\right)$ before derived with only the sign of m changed. The remainder of the process being the same as before, shews that the series is general, or extends to all cases, regard being had to the signs. Q. E. D.

XVII. *A Description of the Anatomy of the Sea Otter, from a Dissection made November 15th, 1795, by* Everard Home, *Esq. F.R.S. and Mr.* Archibald Menzies. *Communicated by* Everard Home, *Esq.*

Read May 26, 1796.

THE subjects from which the following description is taken, were procured from the natives on the west coast of America, near Queen Charlotte's isles, by Mr. ARCHIBALD MENZIES, surgeon in the navy, and naturalist to the expedition fitted out by government for making discoveries, under the direction of Captain VANCOUVER.

The sea otter is not confined to this particular situation; it was met with in the course of the voyage every where along the coast, from 30° to 62° north latitude, and sometimes even an hundred leagues out at sea.

Two sea otters were examined, both of them males; one was a cub not old enough to leave the mother, the other appeared to be full grown.

A Description of the external Appearances.

The large one measured four feet four inches from the nose to the extremity of the tail. The body appears a little compressed, and is nearly of the same thickness throughout; its circumference is two feet four inches and an half.

The colour of this animal varies in different subjects, but in general the head and neck are grey, or of a silver colour; the back, sides, legs, and tail, black and glossy; in some, the longest hairs are tipped with white, which gives them a beautiful greyish cast; the breast and belly also vary from a silver grey, to different shades of light brown. The long hairs shine with a brilliant gloss, but the short fur is exceedingly fine, soft, and thick set; and its colour is either a light chesnut-brown, or it has a silver hue, and a beautiful silky gloss.

In the cub state, the hair is a long, coarse, shaggy fur, of a brown colour, destitute of any gloss; but as the animal grows up the fur becomes finer and more beautiful.

There are two nipples, one on each side of the sheath of the penis, nearer to the anus than to the external orifice of the sheath.

The sheath of the penis does not project beyond the skin of the neighbouring parts; its external orifice is seven inches from the anus, but the sheath itself extends an inch and half further on under the skin of the belly; by which means the penis, when inclosed in it, has its point more effectually defended from injury.

The head is somewhat compressed, and small for the size of the animal. The nose and upper lip are very muscular, and protrude about an inch and half beyond the gums and lower lip. The eyes are small, and placed directly over the angles of the mouth, about half way between the ears and the tip of the nose.

The ears are nearly naked, black, slightly notched at the ends, and about an inch long; they are six inches removed from the tip of the nose.

The whiskers are in great number, they are white and strong, they arise from the upper lip on each side of the nose. There are a few weak long hairs on the eyebrows.

In the upper jaw there are six conical incisor teeth, regularly placed; of these the middle ones are the smallest. Two strong conical fangs, $\frac{3}{4}$ths of an inch long, measuring from the edge of the gums; on each side there are two small obtuse pointed teeth, of which that next the fang is much the smallest; and two broad molares with very irregular grinding surfaces.

In the lower jaw there are four incisores, flatter than those in the upper; two fangs, shorter than the upper ones; and on each side two small teeth and three molares, similar in appearance to those in the upper jaw.

The fore legs are short and strong, with palmated feet; each foot has five toes. They are covered with a thick black fur, which has a fringe of the same colour round the edge of the sole of the foot, where the fur terminates.

The hind legs, when stretched backwards, reach nearly to the end of the tail, and are well adapted for swimming, having five long wide-spreading palmated toes with claws, of which the innermost is the shortest; they measure across eight inches, and are completely covered with fur, except a small spot under the extremity of each toe. The claws are of a light colour, and channelled on the under surface; those on the fore feet are small, and placed so far back that they seem of little use but as a defence for the upper part of the toe; those on the hind feet are stronger, and project beyond the toes.

The tail is flat, and tapers to a sharp point; it is covered with a thicker short fur than any other part of the animal.

A Description of the internal Parts.

The panniculus carnosus, which lies immediately under the skin, is very strong, and extends over the greatest part of the body.

The tongue is four inches long, and rounded at the end, in which there is a slight fissure, giving the tip a bifid appearance. The papillæ on its surface are soft; they are long towards the root, but less so near the tip.

The os hyoides, thyroid, and cricoid cartilages are small for the size of the animal, and weak in their texture. The cricoid cartilage is not a circular ring, but made up of two equal parts, united anteriorly; their lower edge at this union forms an acute angle, the two sides pass a little down upon the trachea as they go round it; and the lower edge laps over the upper annular ring of the trachea.

The thyroid gland is small, and divided into two parts.

The epiglottis is short, and its edges are attached by means of a ligament to the inner side of the thyroid cartilage. The passage of the glottis is small.

The rings of the trachea are circular, and disunited behind, so that their edges meet, and when pressed upon, they lap over each other, being bevelled off for that purpose. Towards the bifurcation of the trachea, the space behind, which is not occupied by the cartilaginous ring, is much increased. This space is occupied by a muscle whose principal fibres are transverse. The trachea is very elastic in a longitudinal direction: seven inches of its length being readily elongated to $10\frac{1}{2}$, and immediately upon being left to itself it contracts to its former state.

The lungs onthe right side have three lobes, two large and

one small azygos lobe; the lower lobe sends a process between the pericardium and diaphragm. On the left side there are two lobes. The lungs were completely empty, so as readily to sink in the spirits in which they had been preserved; the cells are very small, and so elastic that they are difficultly expanded, and readily collapse.

The anterior mediastinum is of considerable breadth, but free from fat, consisting of nothing besides the duplicature of the pleura.

In the fœtus there is a very large thymus gland, convex on its external surface, and concave upon the other.

The heart is inclosed in a thin pericardium, is rather short, free from fat upon its external surface, and rounded at the apex. The ventricles have no communication between them, but the foramen ovale between the auricles remained open; the passage was, however, so oblique, that it must have acted as a valve; it admitted a crow quill. In the fœtus it was less oblique. The structure of the heart, and the valves of the aorta and pulmonary artery, are the same as in other animals. There were no remains of the canalis arteriosus.

The aorta had nothing unusual in its appearance, but the vena cava descendens is very large; when slit open, its breadth is $5\frac{1}{2}$ inches.

The œsophagus is small for the size of the animal.

The stomach is bent upon itself, the pylorus being on a line with the entrance of the œsophagus, and not at a great distance from it. The cardia does not project much into the left hypochondre; and that half of the stomach next the pylorus is much smaller than the other. The coats are thin. The internal surface is free from rugæ; the posterior portion is

smooth, without any appearance of glandular structure; the anterior portion is more vascular and villous. At the pylorus there is an oval part of the internal surface of a dark colour, and rougher or more villous than the rest of the stomach, with a determined edge; the small end of the oval extends about half an inch beyond the pylorus into the duodenum; the larger end goes some way into the stomach, and extends chiefly over the posterior surface, also a little way beyond the great arch anteriorly, covering about half the breadth of this part of the stomach; it is nearly as long again as it is broad. This part is probably glandular; it was only seen in the young subject, which from the smallness of its size was more perfectly preserved, and its internal parts better fitted for anatomical examination. At the pylorus there is the usual thickened valvular appearance.

The stomach was entirely empty, and in a very flaccid state.

The duodenum makes a considerable bend downwards on the right side before it crosses the spine, to become a loose intestine; there is no cœcum or difference of size in the intestines, they are all strung upon the mesentery till within 15 inches of the anus; this part of the gut crosses the spine above the root of the mesentery, and passes down to the anus. The intestines have no valvulæ conniventes; they were 52 feet long, which is 12 times the length of the animal. In a common otter, the intestines are only $3\frac{1}{4}$ times the length of the animal.

In a common otter two bags are found at the anus, but there are none in the sea otter.

The mesentery is 7 inches broad, and its lower part, which may be called meso-rectum, is only five inches in breadth.

The mesentery is thin, and has a great many blood-vessels which are accompanied with fat. There are no lymphatic glands upon the general membrane, but a cluster of very large ones close to the root of the mesentery. The lacteals appear a little larger than in the human subject, but the circumstance of the animal having been two years in spirits, was very unfavourable for their examination.

The omentum is a thin reticular membranous double bag, covering the whole of the intestines; it is attached anteriorly to the great curvature of the stomach, but not to the duodenum; posteriorly to the loins.

The liver is made up of five lobes, besides the lobulus Spigellii; three on the right of the falciform ligament, two on the left.

The gall-bladder is found in the usual situation, is bent in the middle upon itself, and is 6 inches long. The cystic and hepatic ducts unite at the external surface of the duodenum, forming a common canal, or ductus communis cholidochus, about an inch and half long, of an oval shape, with an irregularly rugous internal surface, placed between the muscular coat and the internal membrane of the intestine; it opens into the duodenum by a projecting orifice $2\frac{1}{2}$ inches from the pylorus.

The vena portarum is very large, and the passage behind the ducts of the liver into the cavity of the little epiploon is also large.

The pancreas is situated across the spine behind the stomach, it is not confined within the usual limits, but extends along the posterior membrane of the omentum. It is subdivided into a number of small parts, of an oval shape, all at a certain distance from each other, united by blood-vessels re-

sembling small leaves upon the branch of a shrub. The little pancreas puts on the same appearance, covering the whole meso-duodenum, which is unusually broad. The duct of the pancreas is of the ordinary size, it opens into the duodenum by a separate orifice $1\frac{1}{2}$ inch from the pylorus.

In the common otter the pancreas has not this unusual subdivided appearance, and the duct opens by a common orifice with those of the liver into the duodenum.

The receptaculum chyli is an oval bag, $\frac{3}{4}$ths of an inch broad, from which two trunks go off to form the thoracic duct, each of them about $\frac{1}{5}$th of an inch in diameter; these anastomose frequently in their course, so that there are always two, sometimes three, and even four trunks, running parallel to each other; the thoracic duct is 8 inches in length.

The kidneys are conglomerated, six inches long, and three broad.

The urinary bladder is pendulous and pyramidal, and the ureters open into it very near each other at the lower posterior part.

The testicles are situated under the external skin on each side of the sheath of the penis, but have no pendulous scrotum. They are small, flat, and oval. The tunica vaginalis communicates with the cavity of the abdomen. The cremaster muscle is very strong.

The vasa deferentia, as they pass behind the bladder, become a little convoluted, and open into the urethra at the caput gallinaginis.

The penis, in the relaxed state, is 8 inches long, the bone 6 inches. The corpora cavernosa are small, but strong in their coats. The bone near its anterior end appears to be covered

with a quantity of loose cellular substance; this in the erected state is filled with blood, and forms a large glans six inches in circumference, and four inches long; its anterior extremity is concave, and the end of the bone is seen in the centre.

The penis, when erect, is 11 inches long. The erectores muscles are very strong.

The globe of the eye is extremely small, and the optic nerve is small in the same proportion. Its internal parts were not in a state to bear examination.

The articulation of the lower jaw admits of no motion forwards or laterally; it is a simple hinge an inch long, and very narrow. The condyle of the jaw is so much inclosed in the socket as to be with difficulty disengaged.

The ribs are 14 in number, nine true, and five spurious.

EXPLANATION OF THE PLATES.

Tab. VIII. A side view of the scull of the sea otter, in which the articulation between the scull and lower jaw is different from that in other animals: the condyle of the jaw is almost completely confined in the cavity adapted for it in the basis of the scull, so as to prevent all lateral motion, and make the teeth, when the jaws are closed, always touch upon the same surfaces.

Tab. IX. A view of the basis of the scull, and the lower jaw separated from each other, to show the number and appearance of the teeth; and the peculiar shape of the condyles of the lower jaw, and of the oblong cavities in the basis of the scull with which they are articulated.

Tab. X. fig. 1. A view of the thoracic duct, which in this

tribe of animals is a kind of chain made up of several ducts, and not single, as is more commonly the case in the human body:

a, The receptaculum chyli.

b, The duct which passes over to the left side.

c, A lymphatic vessel from the neck.

The animal's having been preserved two years in spirits prevented the terminations of the ducts in the veins being shown, as they were not filled, the state of the parts not allowing the injection to pass so far.

Tab. X. fig. 2. The penis filled with injection, putting on the same appearance as when erected in the living animal.

aaa, The crura and corpora cavernosa penis.

b, The bone of the penis, seen through the membranes which cover it.

c, The end of the bone in the centre of the glans.

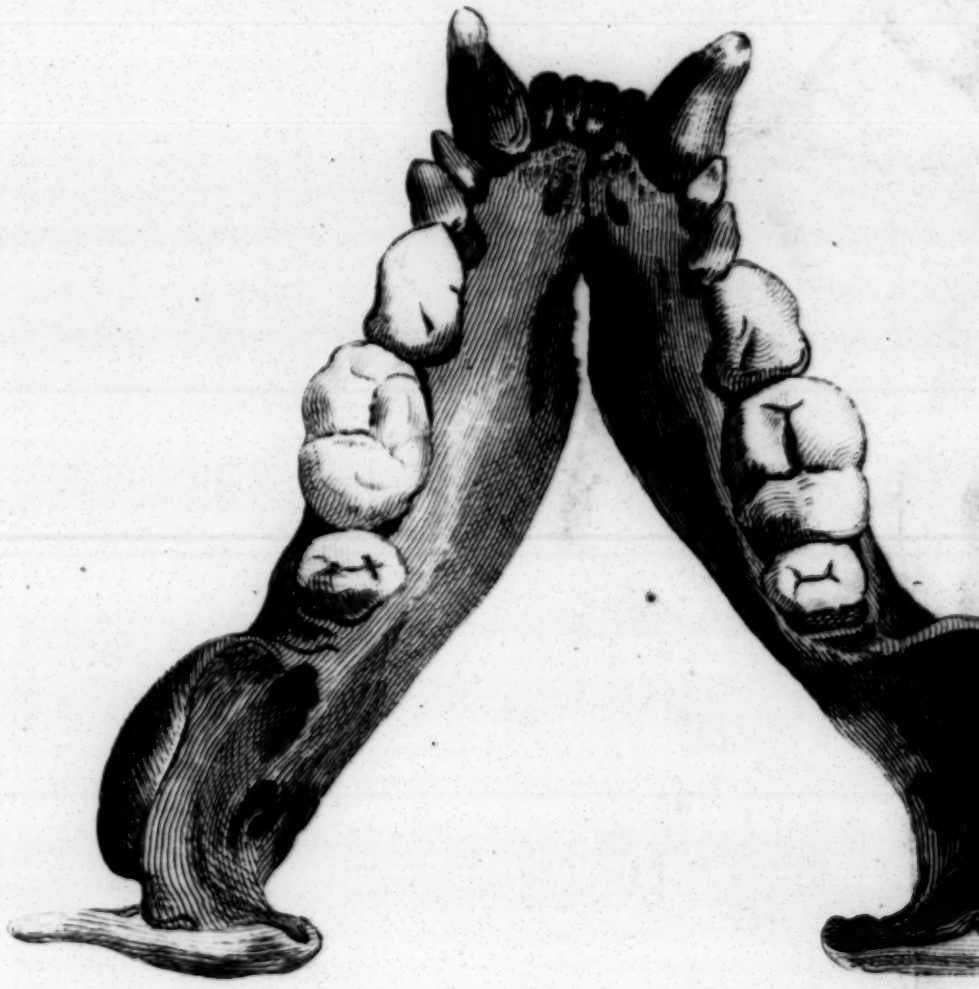
d, The glans penis, which at its anterior extremity forms a cavity with a well defined edge; from the centre of this cavity projects the bone and the orifice of the urethra.

e, A large vein going along the back of the penis from the glans. In the relaxed state the glans entirely disappears, the penis at that part being only the thickness of the bone covered with skin. The substance of the glans is made up of a fine reticular membranous structure, which in the relaxed state of the parts is completely emptied of its blood.

These plates are nearly the natural size of the parts they represent.



Basire Sc.







Bayon. f.


a
c
d

Fig. 1.



Fig. 2.



Basire Sc.

XVIII. *Observations on some ancient metallic Arms and Utensils; with Experiments to determine their Composition.* By George Pearson, *M. D. F. R. S.*

Read June 9, 1796.

THE opportunity of examining the ancient metal instruments, which are the subject of the present paper, was afforded me by Sir JOSEPH BANKS, Bart. K. B. P. R. S. whose zeal for science induced him to sacrifice them to chemical analysis.

I am further indebted to Sir JOSEPH for almost all the curious particulars relative to the history of these instruments; which he was so kind as to permit me to extract from his own notes.

Most of the articles were found in Lincolnshire, in the bed of the river Witham, between Kirksted and Lincoln. Several of them were discovered when that river was scoured out in 1787 and 1788.

The river Witham, between Lincoln and Tattersall, runs through a country almost level, abounding with moorish soil. Its current is slow, and had for ages been depositing gradually a mud, which possesses powers of preserving things lodged in it for a great length of time. The peat moss also of moorish soils has the same property. Inthe Philosophical Transactions, (Vol. XLIV.) an account is given of a body which was dug up in the isle of Axholme that was judged to be of great antiquity,

from the structure of the sandals on the feet; and yet the skin, with hair upon it, was pliable and soft like doe-skin leather.

The workmen who found several of the articles which are the subject of this paper, and at the same time many other arms and utensils of our Roman, Saxon, and Danish ancestors, universally agreed, that most of them lay at the bottom of the river, on the hard soil, and below all the mud. From which observation it may be inferred, that our Saxon ancestors kept the river in much better condition than their successors have subsequently done; and indeed this conclusion receives a great degree of credit from the arms which, in the year 1788, were brought up by an eel-spear near Kirksted Wath; for in that place, and in some few others, the old bottom lay so low, that those who cleaned the river had not occasion to sink down to it, although they removed as thick a body of mud there as elsewhere.

The instruments of which I shall here give an account, were evidently made of what are commonly called brass, and iron. The brass instruments, as I shall show, were allays of copper by tin; and the supposed iron implements were found to be steel.

It will be proper in this place to observe, that brass is a term commonly used to denote any metallic composition the principal ingredient of which is copper; but the most accurate writers in chemistry use the term brass, with more precision, to denote only the compound of copper and zinc; and therefore I shall employ it in this latter sense.

SECTION I. Of the Copper Instruments.

§ 1. *Miscellaneous historical Observations.*

The articles belonging to this head were seven in number; namely, a Lituus, a Spear-head, a Sauce-pan, a Scabbard, and three Celts.

1. *The lituus:* its figure is shown by the drawing, Tab. XI. fig. 1. It is well known to have been a military musical instrument of the Romans. Several classical writers mention it, as Horace, in ode i.

" Multos castra juvant, et *lituo* tubæ
" Permistus sonitus."————————

And Virgil mentions the lituus in celebrating Misenus, who served Hector in the Trojan war, and afterwards Æneas, in the office of trumpeter:

————" Illi Misenum in littore sicco,
" Ut venére, vident indigna morte peremptum;
" Misenum Æoliden, quo non præstantior alter
" Ære ciere viros, martemque accendere cantu:
" Hectoris hic magni fuerat comes; Hectora circum
" Et *lituo* pugnas insignis obibat et hastâ."

Æn. lib. vi. v. 162.

And again in the Georgics, lib iii. v. 182.

" Primus equi labor est, animos atque arma videre
" Bellantum, *lituosque* pati, tractuque gementem
" Ferre rotam, et stabulo frænos audire sonantes."

The lituus is supposed by judicious antiquaries to have been adopted from the barbarous nations; and that the figure of it was intended by the barbarians to resemble a snake, the principal object of their religious worship, and of the most sacred

mysteries of the Druidical religion. If these remarks be true, they throw new light on the crooked staff of the augurs, which the lituus much resembles. It is accurately represented among the trophies which ornament the base of TRAJAN's column at Rome, erected in memory of his conquest of the Dacians and Sarmatians, and covered with bas-reliefs, describing the events of that war. The lituus is also found on the reverses of some Roman coins: see fig. 2.

The specimen before us was found in the river Witham, near Tattersall ferry, in 1768. It is imperfect, a little of both ends being broken off; but notwithstanding these defects it is a very valuable relic, as there is little doubt that it is the only one known to be in any cabinet at this time in Europe. It has been neatly made. The parts which appear like joints are pieces which slide over the tube for ornament, or perhaps for holding the instrument more conveniently. It had the appearance of a brazen tube, from which a great part of a blackish coating had been rubbed off. It was evidently made of a plate of hammered metal of about one-twentieth of an inch thick. The juncture of the edges of the metal, the whole length of the tube, was preserved by means of a solder clumsily applied, by melting it withinside the tube. This solder, which was readily melted out by a red hot iron, was ascertained to be merely tin; for it afforded rapidly oxide of tin by applying nitric acid; the cold saturated solution in muriatic acid afforded CASSIUS' precipitate on dropping into it nitro-muriate of gold; and it afforded no acetite of lead on digesting it in acetous acid.

The black coating was easily scraped off with a knife, but the quantity of it was too small to enable me to determine whether it had been applied by art, or was the accidental

effect of the mud or earth in which it had been buried for many ages. The ancients, as PLINY informs us, stained plates of one sort of copper, the *æs coronarium*, with ox gall to make it look like gold: and the crowns and chaplets of public actors were made of copper so coloured.* It, perhaps, will not appear very improbable, that the coating of the lituus was with this substance.

The performers on the lituus among the Romans, seem to have been persons of at least as high rank as the stage players, and with propriety might therefore use instruments as highly ornamented.

II. Tab. XII. represents a *Spear-head*. In Sir JOSEPH BANKS's collection there is a British spear-head of bone, a Norman one of iron, and a third, the article before us, of copper, which is believed with the greatest reason to be Roman workmanship.† This Roman spear-head is worthy of admiration and imitation, on account of its figure, weight, and size, as an offensive weapon. It is however made of cast metal, as appears from its rough surface, figure, texture, and grain. That it is made of bad metal will be made appear hereafter. It has not been hammered, but has been cast hollow to receive a wooden shaft, and in order to be light and save the expence of metal. It is evident from its figure, that it is of the very best conceivable form for piercing, and for inflicting the largest wound at the least expence of weight and bulk. This weapon was found in the river, with another, near Fiskerton, in the year 1788.

* "Coronarium tenuatur in laminas, taurorumque felle tinctum, speciem auri in coronis histrionum præbet." PLINY, lib. xxxiv. cap. viii.

† An instrument is described and represented by a figure in the Archæologia, Vol. IX. fig. *c*, exactly like this spear-head, and it is deemed to be Roman.

III. The *Sauce-pan* is represented in Tab. XIII. From its form and the grain of its fracture, and its being one entire piece, it appears to have been made of cast metal. It is considered to be a piece of Roman workmanship. It is neatly and curiously grooved at the bottom, to admit the fire to penetrate to the contents more easily. On the handle is impressed, seemingly with a stamp, C ARAT; which letters may possibly signify CAIUS ARATUS, as the latter part of the stamp seems not to have made an impression.

It was found in the year 1768 in the river Witham, near Tattersall ferry. It appeared to have been tinned, but almost all the coating had been worn off. As it was said that it had been used by some boatmen, for some time after it had been found, it might have been tinned after it got into their possession. The art of tinning copper, however, was understood and practised by the Romans,* although it is commonly supposed to be a modern invention, therefore it is not very improbable that this utensil was originally covered with tin by that people.

IV. Tab. XI. fig. 3. represents the *Scabbard* with a sword of iron within it. This and another brass scabbard in Sir JOSEPH BANKS's collection were found in the river Witham in 1787, near the site of Bardney abbey. In the same place were found many other arms; from whence some conjecture respecting their antiquity may be formed. We may suppose that the destruction of that noble monastery was not effected without loss on the side of the Danes who attacked, and the Saxons who defended it. By the best accounts it was destroyed in the year 870.†

* "Stannum illitum æneis vasis, saporem gratiorem facit, et compescit æruginis "virus." PLINY, lib. xxxiv. cap. xvii.

† TANNER's *Notitia Monastica*, p. 248.

These swords are therefore prior to the conquest; but which are Danish and which are Saxon, is not easy to determine. Probably these people, who were always at war with each other, and consequently had frequent opportunities of possessing themselves of each other's weapons, adopted from each other whatever improvements were made by either; and thus in effect they might both use the same weapons. Swords like this are to be seen in illuminated manuscripts, and on painted glass. In STRUTT's Þorpa Ænzel-cynnan, where he describes the customs of our Saxon and Danish ancestors, such swords frequently occur; especially in the lives of the two OFFAS. Similar swords are also in the hands of the Danes, who are killing the abbot of Croyland on the shrine, as delineated by Dr. STUKELEY, in the Philosophical Transactions, Vol. XLV. p. 597.

The brass scabbard before us possesses some degree of elegance, and much accuracy of workmanship. It appears to have been originally covered with a bright blue varnish, but the quantity was much too small for ascertaining its nature. Exactly such a sword as this is represented at the side of a Danish soldier, in pl. 26, Vol. I. of STRUTT's work just quoted. The sword within this scabbard was destroyed by rusting, and could not be drawn out. The pommel and guard had been broken off. There was a plate of open work, about four inches long, laid over one side, and near the top of the scabbard; and at the bottom, on one side, was a sort of joint; and on the other and opposite side was a bas-relief figure. The scabbard was made of hammered metal, and was perhaps about one-thirtieth of an inch thick.

The next, and last three articles under the present head, are

known to antiquaries by the name of *Celts*. They were, probably, instruments used by the ancient Britons, Gauls, or Celtæ. The learned do not agree whether the celts were Roman workmanship or not: nor to what particular uses they were applied. Accordingly some persons have supposed that they were the offensive weapons of our ancestors; and others have supposed that they were both offensive military weapons, and civil instruments; but the most probable opinion is, that they were merely domestic tools. Many of the celts are cast after the model of stone implements, which are confessedly ancient British or Celtic chopping instruments, and tools for making holes. Several of these stone implements, in Sir JOSEPH BANKS's collection, correspond exactly with the figure and size of the celts. Great quantities of these instruments have been at different times discovered in England, as well as in Ireland, and some few in France. Sometimes they have been found in heaps, as if the owner had, and probably did throw them away by basketfulls, as things of little value. It has been very ingeniously conjectured, that when the Romans came to Britain they found the inhabitants, especially to the northward, very nearly in the same state as that in which our late discoverers found the natives of the South Sea islands. The Britons parted with their valuable articles of food, rarities, and commerce, for metal tools made in imitation of their stone ones; but in time, finding themselves cheated by the Romans, who made these tools of bad metal, of the shape of the ancient British stone axe, as the inhabitants of Otaheite were by the use of base metals; they relinquished these tools when they became acquainted with those made of better metal, and according to the Roman patterns. Hence we see a reason for such great

quantities of celts being found among the Celtic nations, and not among the Roman, excepting now and then a specimen, which may be looked upon as the tool, or spoil of barbarian auxiliaries.

v. Tab. XIV. fig. 1. represents a *Celt*, No. 1. found on the peninsula of Ballrichen, within the precincts of a Druidical grove, or dwelling, in Ireland. The same kind of celt is described in WRIGHT's Louthiana, b. 14. p. 7. pl. i. and also in the Archæologia, Vol. V. p. 113. by Dr. LORT. It weighed one pound and one quarter. Except at the edge it was nearly three-eighths of an inch in thickness. It was of a blackish colour, from oxide of the metal and dirt upon its surface.

VI. Fig. 2. represents the *Celt*, No. 2. It was found in a field, by ploughing, in Cumberland. The celt in Dr. LORT's collection which most resembles this article is delineated by fig. 11. pl. viii. p. 113. Vol. V. of the Archæologia. The celt before us differs from that just referred to, in being grooved on both sides to receive a shaft or handle, instead of having a socket. It weighed nearly three quarters of a pound, and was about five-eighths of an inch thick, except at the edge. Its external appearance was like that of the former celt.

VII. Fig. 3. represents the *Celt*, No. 3. It was much smaller than the two former, weighing only about five ounces, but it resembled in shape fig. 1.

§ 2. *External, or more obvious Properties.*

(*a*) These metallic instruments differed considerably from one another in their external appearance, with respect to colour. A little green or blackish oxide of copper adhered to their surfaces. The lituus, and the celts, No. 1. and No. 2. in

the parts not covered with oxide or dirt, were yellow, like rather pale brass; the scabbard was somewhat less yellow; and the spear-head and sauce-pan were of a pale yellowish-brown colour. The celt, No. 3. was covered entirely with black oxide and other extraneous matter.

(*b*) A small part of the surface of each of the articles being filed or rubbed to remove the exterior matter, the colours of all of them were what I would call different shades of pale copper colour; not very different from gun metal and prince's metal, but not at all like brass. They all took a fine polish, which, when first produced, was pale coloured and whitish; but, by exposure to the air, it became deeper coloured and tarnished. On examination of the polished surfaces with a lens, the metallic matter appeared perfectly homogeneous, and of a close texture.

The articles being cut through with a chizzel, the same differences among the cut surfaces appeared, as among the polished surfaces just described. On fracturing the metallic bodies, the visible interior structure or texture did not appear similar in colour and grain.

The *spear-head* was open grained, almost as copper; and porous, as if made of bad metal, but of a blackish-brown, or dark grey colour.

The *sauce-pan* was also open grained or porous, although less so than the spear-head. The colour of the grain, like that of the spear, was dark brown or grey.

The *lituus* and the *scabbard* were close grained; lighter coloured than the former, but grey.

The *celts* were all open grained, but much less so than the pan and spear-head; and differed from them in being brown.

The celts when lacerated exhibited shining facets, and striæ or radii. The cut surface, and also the grain of the celt, No. 3. was much paler than the cut surface and grain of the other two celts.

An experienced observer can judge tolerably well concerning many metals, and metallic compositions, by inspecting fractured surfaces; but to judge accurately from these appearances, the metals to be compared with one another should be in the same state of aggregation.

I therefore melted the old implements, and cast them into the same ingot mould. In this state I could also judge better of their hardness, brittleness, strength, malleability, and other properties, but especially of their specific gravity.

Each of these ingots was fractured by a pretty smart stroke with a hammer. The fractures of all the metals were, in this state, close, or fine grained; and therefore denoted hardness. Their appearances were the following.

1. The *celt*, No. 1. Less close grained than the rest, and pale brown.

2. The *celt*, No. 2. Fine grained, and greyish brown.

3. The *celt*, No. 3. Still finer grained; bright greyish, and somewhat crystallized.

4. The *scabbard*. So like the celt, No. 2. that it was not easy to perceive a difference between them in grain and colour.

5. The *spear-head*. Close grained as any of the ingots; but of a dull pale slate colour.

6. The *sauce-pan*. Fine grained, but not so much so as the *lituus*; and of a somewhat slate-coloured hue, or dark grey; but less dull than the scabbard.

7. The *lituus*. Very close grained; of a bright slate and silvery hue, and partly crystallized.

(*c*) The trial with the drill, and with the hammer, showed the lituus to be the hardest of these old metals; almost as hard as bell metal: the next in hardness was the celt, No. 3: the next in order was the pan: and next the celts, No. 1, and No. 2, and the spear-head, which were nearly alike in this property.

They were all much harder than copper, or even than brass.

(*d*) The ingots of the lituus, sauce-pan, spear-head, scabbard, and the celt, No. 3, all broke readily enough with a smart stroke with the hammer, and more readily than the ingots of the celts No. 1. and No. 2. Plates of the spear-head and pan possessed little malleability and ductility; being very liable to be cracked in beating them, as if they had been made of impure metal. The lituus was very flexible, malleable, and elastic; so were plates of the celts, No. 1, and No. 2, and of the scabbard; but the celt, No. 3, although, apparently, made of the purest metal, possessed little malleability. They were all less malleable than brass. The celts, No. 1, and No. 2, possessed more strength and hardness conjointly than any metal in use, except iron and steel; and of course are the fittest metal for chopping instruments of any known metal, except iron and steel.

(*e*) The metallic implements before us were all rendered harder, and specifically heavier, by hammering: and they were again rendered softer, and specifically lighter, by annealing, that is, by ignition and gradually cooling.

(*f*) These ancient metals were very sonorous, particularly the sauce-pan, the lituus, and the celt No. 3.

§ 3. *Specific Gravities.*

These were ascertained by weighing the metallic instruments themselves, as well as after melting and casting each of them in the same ingot mould.

1.	The lituus, before melting — —	8,3
	Ditto, after melting — —	8,85
	Ditto, another piece, after melting	8,888
	Ditto, another piece, after melting	8,650
2.	The spear-head, before melting —	7,795
	Ditto, after melting —	8,805
	Ditto, another piece of ditto —	8,850
	Ditto, another piece of ditto —	8,830
	Ditto, another piece of ditto —	8,860
3.	The sauce-pan, before melting —	7,960
	Ditto, another piece — —	8,255
	Ditto, after melting — —	8,630
	Ditto, another piece of ditto —	8,769
	Ditto, another piece of ditto —	8,740
4.	The scabbard, before melting —	8,5
	Ditto, another piece — —	8,6
	Ditto, after melting — —	8,657
	Ditto, another piece of ditto —	8,875
5.	The celt, No. 1. before melting —	8,780
	Ditto, another piece — —	8,885
	Ditto, after melting — —	8,734
	Ditto, another piece of ditto —	8,771
6.	The celt, No. 2. before melting —	8,680
	Ditto, another piece — —	8,653
	Ditto, after melting — —	8,474
	Ditto, another piece of ditto —	8,600

7. The celt, No. 3. after melting	—		8,854
Ditto, another piece	—	—	8,600

§ 4. *Experiments with Fire.*

(*a*) These old instruments melted at a lower temperature than that at which copper, or even some kinds of brass melt.

Although I did not succeed in determining precisely the temperature at which each of them fuses; it may be useful to relate the experiment made with that view.

(*b*) 100 grains of each of the above seven ancient metallic instruments, and the same quantity of copper, of pure silver, of allay of copper with one-eighth of its weight of tin, of allay of copper with one-tenth of its weight of tin, of allay of copper with one-twentieth of its weight of tin, of allay of three parts of copper with one of zinc, and of gun-metal, were exposed each in separate coppels, under a muffle, to the greatest degree of fire which I could produce in the best assay furnace.

A pyrometer clay piece of WEDGWOOD's instrument was also put into each coppel.

During forty minutes exposure to fire, not one of the metals melted, except the pure silver,* and the allay with zinc: nor did any of them emit visible vapour, or inflame, except the allay with zinc; nor did any matter ooze out of any of the metals.

* In WEDGWOOD's scale it is stated, that pure silver melts at 28°, and Swedish copper at 27°. But every part of the furnace in the above experiments might not be of the same temperature for the same space of time: and perhaps the state of cohesion and figure of the metal exposed to fire may account for the difference in the degree noted by the pyrometer in my experiment, from that stated in the scale. For I am assured, by Mr. THOMAS WEDGWOOD, that the degree of contraction is uniform among a number of pyrometer pieces, exposed in the same part of the furnace at the same time.

On cooling, it was found that the figure of the metals which had not been melted in the coppels was not altered, but they were changed, either totally or externally, into scoria-like black matter. The copper allayed with zinc was found to contain a nucleus of copper within a large proportion of black scoria and white oxide of zinc. The celt metals were changed into scoriæ, including copper-like metal. The other old metals were changed entirely into scoriæ. The copper allayed with one-twentieth of its weight of tin was changed into scoria containing a little copper; but the copper allayed with one-eighth of tin was changed into scoria containing a little copper, seemingly allayed with a much smaller proportion of tin than before.

The pyrometer pieces indicated degrees of fire, which varied between 18° and 21°. The pyrometer piece in the coppel which contained the silver, and also that in the coppel which contained the copper, denoted 20° of WEDGWOOD's scale, or about 3800° of FAHRENHEIT's scale.

(*c*) A thin plate of each of the old metals being exposed to the flame of a candle with the blow-pipe, a blue and green flame appeared, which soon disappeared, although the fire of the candle was applied so as to keep the metal red hot.

The same kind of blue and green flame was emitted from plates of these metals when they were exposed to fire in open crucibles, before they were melted; but it disappeared in a few seconds of time, although the fire was continued to be applied to keep the metal red hot; nor was any such flame produced when the metal was melted in open vessels, or kept stirring when in a fluid state.

(*d*) Each of the ancient metals being melted in close vessels, was then exposed to the air, and stirred with an iron rod;

but none of them emitted any blue flame, or white vapour, as was the case when brass was so treated.

The following experiment, to determine whether the ancient metal instruments contained any gold or silver, was made, while I was present, by Mr. BINGLEY, Assay Master.

(*e*) 50 grains of each of these metals, and as much gun metal, and also the same quantity of brass, were put into separate coppels, together with 150 grains of lead, under the muffle of an assay furnace: 150 grains of lead were also put alone, by way of test, into a separate coppel.

The fire being kept up in the usual way, the brass emitted a blue flame, and began to melt, discharging at the same time white fumes; but soon after it was melted the flame, and white fumes, disappeared. The ancient metals, and also the gun metal, afterwards melted, and without sending forth any flame, but a slight fume was seen when they were in fusion; which was particularly evident from the coppel containing the spear-head metal. This fume was not seen to arise from the coppel which contained lead only; but the Assayers observe it from charges of lead with silver, or lead with gold and silver, when much air is admitted.

The process being finished, nothing was left in the coppels which contained lead only, and lead and brass, except a just visible particle of silver; but in the other coppels there remained about one-third of the original quantity of the ancient metals, and of the gun metal: and therefore into each of these coppels 150 grains of lead were again introduced. The process being performed a second time, every particle of metal was absorbed, excepting a just visible particle of silver in the coppels which contained the celt, No. 2, the metal of the scabbard, and

the gun metal: but there was a much larger globule of silver in the coppel which contained the spear-head metal.

As the only metal which appeared to contain more silver than the test itself was the spear-head, and as it emitted more fume than the rest, I repeated the process on this metal.

The process of cupellation the second time, as before, caused the appearance of the white fume, and afforded a residue of silver, as before, in greater quantity than that of the test. The silver was determined in the most accurate way to amount to the proportion of fifteen grains in a Troy pound of the spear-head metal. There was no gold in this silver, for it dissolved totally in nitric acid.

§ 5. *Experiments with nitric Acid.*

(*a*) A polished piece of each of the ancient metals was just wetted with *nitric* acid. Fumes of *nitrous* acid arose, and the part wetted became *white* and corroded; as is the case when the nitric acid has been applied in this manner to the allay of copper by tin.

(*b*) On 300 grains of each of the above metals, in a small retort, were poured 1800 grain measures of nitric acid, purified by distillation from nitrate of silver, and of the specific gravity of 1,350. The hydro-pneumatic apparatus being affixed, generally from thirty to forty ounce-measures of nitrous gaz came over in the cold, in the course of two to three days. In this time the whole, or at least the greatest part of the metal, was oxidified and dissolved; there being a clear blue solution, with a copious white sediment, and sometimes a part of the undissolved metal.

By means of the fire of a lamp, more gaz came over, which

had been absorbed by the solution, and which also was afforded by the dissolution of the remaining metal.

The whole quantity of nitrous gaz varied, with the same as well as with different metals, between 60 and 85 ounce-measures; but either from my own inability to observe, or from the circumstances on which this variety depended being unknown, I cannot explain the reason of such differences in the result.

(*c*) After the solution (*b*) had stood several days, the clear blue liquor was decanted, and filtrated, from the white sediment: and pure water was poured upon the filter repeatedly, till what passed through was colourless, and almost tasteless. The filtrated liquid was boiled to evaporate all but about six ounces; and it deposited, on standing, a small quantity of white sediment.

The white sediment, from the solution (*b*), being dried, amounted to the following different quantities, from 300 grains of each of the different metals, namely,

1. The sauce-pan, exclusive of a little dirty extraneous matter, – – 65 grains, or $21\frac{1}{2}$ *per cent.*
2. The spear-head, exclusive of a little dirty extraneous matter, – – – 63 grains, or 21 *per cent.*
3. The celt, No. 3. – 55 grains, or $18\frac{1}{2}$ *per cent.*
4. The lituus, – – 54 grains, or 18 *per cent.*
5. The scabbard, – 48 grains, or 16 *per cent.*
6. The celt, No. 1. – 42 grains, or 14 *per cent.*
7. The celt, No. 2. – 42 grains, or 14 *per cent.*

(*d*) The decanted and filtrated liquid (*c*) being duly evaporated to crystallization, was found to contain nothing but nitrate of copper, and sometimes a very minute portion of

white sediment; for it threw down nothing but prussiate of copper, on adding prussiate of soda; nor was any silver deposited on immersing in it bright copper wire; nor was any precipitation occasioned by adding muriatic acid, or muriate of soda, to the concentrated blue solution.

(*e*). The white sediment (*c*) was a light impalpably fine powder: it had a little metallic taste: it could not be melted with borax by flame with the blow-pipe, but was diffused through that salt, and rendered it opaque.

This sediment dissolved totally, except a little mere dirt, by long digestion in muriatic acid, and immediately in this menstruum when caloric was applied to make it boil.

This solution in muriatic acid did not throw down Cassius' precipitate on adding to it nitro-muriate of gold, but afforded a white deposit exactly like that which is made on adding nitro-muriate of gold to muriate of tin, made either by boiling tin in a large proportion of muriatic acid, or by dissolving oxide of tin (made with nitric acid) in muriatic acid.

The muriatic solution of the white sediment (*c*), on adding prussiate of soda, afforded a precipitate exactly like that which appears on adding prussiate of soda to muriate of tin.

The white sediment (*c*) being mixed with tartar, upon charcoal, the flame of a candle by the blow-pipe was directed upon it; by which treatment small silver-like globules were made to appear. These globules being collected, were digested in the cold, in so small a proportion of muriatic acid as could not dissolve the whole of the globules supposing them to be tin. They were gradually almost all dissolved, and nitro-muriate of gold being added, Cassius' precipitate was immediately deposited. But the metallic globules being dissolved by boiling in a large proportion of muriatic acid, no Cassius'

precipitate was produced on adding nitro-muriate of gold; nor on adding it to tin dissolved by boiling in a large quantity of muriatic acid.

The preceding analytical observations and experiments will, on examination, perhaps be found to contain sufficient evidence to demonstrate that each of the ancient metallic instruments contains copper and tin; and they will also perhaps be found to prove, that these metals contain no other kind of metal, or other species of matter. But, in order to ascertain the proportion of the tin to the copper more accurately than I was able to do by analysis, and also in order to confirm or invalidate the evidence of analysis, I made the following synthetical observations and experiments.

§ 6. *Synthetical Observations and Experiments.*

Experiment 1. 50 grains of tin were united by fusion with 1000 grains of copper. The ingot of this allay of twenty parts of copper by one of tin, when polished, differed from the celt metals in shade of the same colour; these being much paler than this allay. It was a good deal harder, and not so tough as copper, but it was not so hard, and was more tough than the celt metals. Its fracture shewed also a more open grain than the old metals, and more inclining to the peculiar red colour of copper, instead of the brown and grey, or slate colour of the ancient metals.

With nitric acid it afforded, like the ancient metals, a blue liquor, and white deposit of oxide of tin; but in much smaller proportion than any of them; not being more than seven *per cent*.

Experiment 2. 100 grains of tin were united by fusion with 1500 grains of copper. This allay of fifteen parts of

copper with one of tin resembled the celt metals, No. 1 and No. 2, in colour, polished surface, grain of the fracture, and brown colour of the fracture: consequently the red colour of the copper was completely destroyed. It was not, however, so hard, and was stronger than these celt metals; but was harder than the spear and the sauce-pan.

The solution of this allay with nitric acid only differed from that in the former experiment in affording a more copious white deposit, namely, ten *per cent.* of it in its dried state.

Experiment 3. 100 grains of tin were melted with 1200 grains of copper. This allay of twelve parts of copper by one of tin could scarcely be distinguished from the last described allay in the colour of the polished surface, nor was it so much closer grained or lighter coloured in its fracture as might have been expected; nor could I by the hammer distinguish it from that allay in point of hardness and strength. On the trial with the drill, it however betrayed a good deal more hardness. It was almost as hard as the celts, No. 1 and No. 2.

With nitric acid it afforded a deposit of eleven *per cent.* of oxide of tin.

Experiment 4. 100 grains of tin were united by fusion with 1000 grains of copper. This allay of copper with one-tenth of its weight of tin was as pale coloured as the celts, No. 1 and No. 2, but not nearly so pale as the celt, No. 3. I could not distinguish this allay in the properties of hardness and strength from the two celts, No. 1 and No. 2, and the scabbard; but it was harder than the spear-head and sauce-pan, and not so brittle. Its fracture showed the same kind of rather open grain, and texture, as that of the celts, No. 1 and No. 2, be-

fore they were melted, but it was not so close grained as any of the ancient metals after fusion; and it differed from all of them in being of a lightish brown colour.

The solution in nitric acid differed only from the former in affording a greater proportion of white deposit, namely, thirteen grains and a half *per cent.*

Experiment 5. 900 grains of copper were melted with 100 grains of tin: which allay of nine parts of copper with one of tin differed very little from the former.

By means of nitric acid this allay gave seventeen grains *per cent.* of oxide of tin.

Experiment 6. 100 grains of tin were melted with 800 grains of copper. This allay of eight parts of copper with one of tin was also scarcely distinguishable from the two former allays, in colour, strength, appearance of fracture, texture, and polish.

With nitric acid this allay afforded eighteen grains and a half *per cent.* of oxide of tin.

Experiment 7. 100 grains of tin were melted with 700 grains of copper. This allay of seven parts of copper with one part of tin was evidently different from any of the former allays; being harder, more brittle, paler coloured, the fracture showing a much finer grain, and of a grey or somewhat slate-colour. The grain, therefore, of this allay resembles in colour that of the celt, No. 3, the lituus, the spear-head, and the scabbard. It was especially like the lituus and the celt, No. 3, in the rather bright and silvery appearance of the fracture, instead of the dull slate colour of the spear-head and sauce-pan. On trial with the hammer, and the drill, it resembled exactly the lituus in brittleness and hardness. It was a little harder and

more brittle than the celt, No. 3, and of course much more so than the other ancient metals.

This allay, on solution in nitric acid, yielded twenty *per cent.* of oxide of tin.

Experiment 8. 100 grains of tin were melted with 600 grains of copper. This allay of six parts of copper with one of tin was harder than any of the above allays: and perhaps it was harder and more brittle than any of the ancient metals. Its fracture exhibited a still finer, brighter, silvery, and more crystallized grain than any of the preceding allays.

Nitric acid separated from this allay twenty-two *per cent.* of oxide of tin.

Experiment 9. 100 grains of tin were melted with 400 grains of copper. This allay of four parts of copper with one of tin was about as hard and brittle as some sorts of bell-metal. Its fracture was still paler, finer grained, and silvery, than any of the preceding allays.

Nitric acid separated from this allay twenty-seven *per cent.* of oxide of tin.

Experiment 10. 100 grains of tin were melted with 300 grains of copper. This allay of three parts of copper with one of tin, was much harder than any of the preceding ones. It was also much more brittle, the fractured surface was quite smooth, and without almost any grain at all. It was of a silvery hue, and resembled much an ingot of a melted bell; excepting that it was finer grained, and of a duller colour.

Experiment 11. 100 grains of tin were melted with 200 grains of copper. This allay of two parts of copper with one of tin, was as brittle almost as glass. The fracture showed no

grain at all, being quite smooth. Its colour was more like that of silver than any other metal.

Experiment 12. The metal of which what are called *brass guns* are made, does not in general contain a grain of zinc. They are made of an allay of about ten to twelve or thirteen parts of copper, with one part of tin. I found that the shavings of one of these guns melted much more readily than copper. The ingot was not so hard, but tougher than any of the above ancient metals. It possessed nearly the same hardness and strength as the allay, in *Experiment* 3. of twelve parts of copper by one of tin. The colour of the polished surface, and the grain and colour of the fractured surface, resembled pretty exactly that allay. Of course this gun metal is only a little less hard and brittle than the celts, No. 1 and No. 2, but it resembles them very exactly in the colour and texture of the grain.

This gun metal afforded nearly thirteen *per cent.* of oxide of tin, by means of nitric acid.

Experiment 13. 20 grains of tin and 10 grains of zinc were melted with 800 grains of copper. This allay of eighty parts of copper with two parts of tin, and one part of zinc, was a metal which had a very different aspect when polished, as well as when fractured, from either copper, or any of the above allays, or any of the ancient metals. For it had a rich yellowish or golden hue, and was nearly as tough, but a little harder than copper.

Experiment 14. 20 grains of zinc were united by fusion with 800 grains of copper. This allay of forty parts of copper with one part of zinc, was of a yellowish golden hue, and of

course was very different in its external appearance from the allays of copper by tin. Like the allay of *Experiments* 1st and 13th, it was too soft, and, as the artists term it, *clingy*, to receive the impression of lines, figures, and letters, or for instruments in which holes are to be drilled.

The solution of this allay in nitric acid was blue, like those of the preceding allays and old metals, but there was no white deposit.

Observation. This is the proper place for me to observe, that all the above allays, and the gun metal, melted at a lower temperature than copper does; and, as far as I could determine, the temperature of fusion decreases as the proportion of tin increases.

The next experiments were made not only to satisfy myself, that if *iron* had been an ingredient in the ancient metals, it must have been made appear by the test employed; but also to determine the question made by some chemists, whether copper can be allayed by iron; and to show, as others have asserted, the allays of copper by iron, which were employed by the ancients.

From the writings of many able chemists I was inclined to suppose, that a malleable uniform metal could not be composed of copper and iron, without the aid of an intermede. I therefore, in the first place, used tin as the intermede.

Perhaps some of these experiments next to be related may not be found immediately relative, but as they occurred in the course of investigation, and as I believe few experiments of the same kind have been published, perhaps they will be found useful.

Experiment 15. 2000 grains of tin were melted with 1000

grains of steel,* by keeping the two metals in a close crucible exposed to a pretty fierce fire of a melting furnace. An allay was produced of an uniform metallic mass, of the colour of pewter, of a very open grain, but uniform texture; which was as brittle, and not harder than certain kinds of old bad pewter.

Experiment 16. 1800 grains of tin were melted with 600 grains of steel. This allay of three parts of tin with one of steel was perfectly similar to the last allay of two parts of tin with one of steel, excepting that the allay of this experiment was not so hard, and was less brittle.

Having thus prepared the steel for union with copper, by the medium of tin, I added to it copper.

Experiment 17. 600 grains of the allay of *Experiment* 15. were melted with 2400 grains of copper. This allay of twelve parts of copper with two parts of tin, and one part of steel, resembled exactly the allay of six parts of copper with one of tin, in *Experiment* 8. in the colour and grain of the fracture; in its polish; hardness; and brittleness. Its fracture was of course of a slate-coloured hue, or dark grey, somewhat crystallized and silvery. The fracture being inspected with a lens, the grain appeared finer or shorter than that of the allay of six parts of copper with one of tin.

The solution of this metal in nitric acid produced nitrous gaz, a blue solution, and a white deposit; as occurred in the dissolution of the ancient metals, § 5. p. 411, and of the allays of copper with tin, p. 414—419; but the result of the examination of this blue solution and white deposit was different from

* The steel employed was part of a file. Steel was preferred to iron, because it is fusible, but iron is not.

that of the ancient metals, and satisfied my mind completely, that if those metals had contained iron it must have been detected.

(*a*) The blue solution of this experiment being boiled, to carry off redundant acid, and evaporate about three-fourths of its water, prussiate of soda was added. A reddish-brown precipitation ensued, which resembled exactly that produced by adding this test to nitrate of copper.

(*b*) The white deposit of this experiment having been well edulcorated by pure water, was wholly dissolved in muriatic acid. This solution differed from that of all the white deposits of the preceding experiments, in being of a reddish-brown colour, like dilute solution of muriate of iron, and especially in affording a copious precipitation of prussiate of iron by prussiate of soda. With nitro-muriate of gold, however, this solution only produced a slight grey precipitation, as in the former experiments.

Experiment 18. 1000 grains of the allay of *Experiment* 15. were melted with 2000 grains of copper. This allay of about seven parts of copper with two parts of tin, and one part of steel, was an extremely hard metal, much harder than that of the last experiment; and it was very strong, but scarcely malleable. It took a beautiful polish, of a silvery colour. It was of a perfectly homogeneous texture. The grain of its fracture was extremely fine and uniform, and of a grey colour.

Experiment 19. 2000 grains of copper were melted with 200 grains of steel, in a close vessel, by keeping them exposed to a fierce fire in a wind furnace for about twenty minutes. This allay of ten parts of copper with one part of steel, was of a copper colour. The grain of its fracture was coarse, like that of copper. It was harder than copper, and less tough,

but quite malleable. It was about as hard as the allay of twenty parts of copper by one of tin, and consequently was not nearly so hard as the softest of the ancient metals.

Experiment 20. 1000 grains of copper with 500 grains of a small round steel file were exposed to fire, as stated in the last experiment. On opening the crucible, part of the steel only was found to have been melted and united to the copper; but the other part of the steel which retained its form, was thoroughly impregnated or penetrated by copper; so that on breaking the part which had not been melted, and which was very brittle and porous, it was in appearance imperfectly metallized copper. The part of the allay which had been melted was not, as far as I could perceive, different from the allay of the last experiment, except that it was a little harder; being thought to be about as hard as brass.

I have not hitherto set down the specific gravities of the above allays, because I thought it most useful to make the statement of them together in one column, with, at the same time, the specific gravities of different parcels of copper, tin, zinc, brass, and bell-metal.

Specific Gravities.

1.	Plate of copper, a little hammered	8,8
	Another specimen, a little hammered	8,904
	Copper ingot	8,418
	Another specimen of ditto	8,414
	Shotted copper*	8,08
2.	Tin	7,3

* By shotted copper is meant copper which has been poured when melted into cold water, by which it is divided into small globular pieces and grains.

	Another specimen of tin - - - -	7,25
3.	Zinc - - - - -	7,171
	Ditto, another specimen - - -	7,150
4.	Allay of copper with $\frac{1}{20}$ its weight of tin; three different parts of the same ingot - -	8,764
		8,760
		8,611
5.	Allay of copper with $\frac{1}{15}$ its weight of tin, in the state of ingot - - - - -	8,760
6.	Allay of copper with $\frac{1}{12}$ its weight of tin -	8,700
	Ditto, another piece of the same ingot - -	8,677
7.	Allay of copper with $\frac{1}{10}$ its weight of tin; two specimens of the same ingot - - -	8,658
		8,671
8.	Allay of copper with $\frac{1}{9}$ its weight of tin; two specimens of the same ingot - - -	8,510
		8,6
9.	Allay of copper with $\frac{1}{8}$ its weight of tin; two specimens of the same ingot - - -	8,790
		8,711
10.	Allay of copper with $\frac{1}{7}$ its weight of tin; two specimens of the same ingot - - -	8,766
		8,530
11.	Allay of copper with $\frac{1}{6}$ its weight of tin, in one large piece, as it cooled in the crucible -	8,635
12.	Allay of copper with $\frac{1}{4}$ of its weight of tin; two different parts of the same ingot - -	8,966
		8,928
13.	Allay of copper with $\frac{1}{3}$ of its weight of tin; two different parts of the same ingot - -	8,944
		8,909

14.	Copper allayed with $\frac{1}{4}$ of its weight of zinc -	8,400
15.	Common brass ingot - - - -	8,300
16.	Common brass, hammered - - -	8,500
17.	Piece of an old small bell, probably compounded of copper with $\frac{1}{4}$ its weight tin - -	9,
18.	Two different parts of the ingot of gun metal of *Exp.* 12. p. 418 - - -	8,5
19.	Allay of 80 parts of copper with two of tin, and one of zinc - - - -	8,560
20.	Allay of 40 parts of copper with one of zinc -	8,450
21.	Allay of 2 parts copper with one of steel -	7,370
22.	Allay of 3 parts of tin with one of steel -	7,214
23.	Allay of 12 parts of copper with two tin, and one of steel; different parts of the same ingot -	8,534
		8,666
		8,633
		8,720
24.	Allay of 7 parts of copper with two of tin, and one of steel - - - - -	8,633
25.	Allay of 10 parts of copper with one of steel; two different parts - - - -	8,655
		8,600

§ 7. *Conclusions and Remarks.*

1. The first conclusion from the preceding observations and experiments is, that the ancient metal instruments examined consist principally of copper, as appears; 1st, from their external and obvious properties; particularly their colour, taste, malleability, and specific gravity: 2dly, from the whole of the metals, except a small deposit, yielding nitrate of copper with nitric acid: 3dly, from the synthetic experiments.

II. I conclude that these metal instruments contain tin; which metal was made appear, by the experiments on the white deposit afforded on dissolution in nitric acid, § 5: and which also was made appear by the synthetic experiments, § 6.

III. The third conclusion is, that these metallic instruments consist of metal only, or at least of nothing else which can be detected by ordinary known modes of analysis: for they are all malleable, and uniform in their texture; which properties metals do not possess when they are mixed by fusion with extraneous substances hitherto discovered by analysis; except carbon in several metals, and siderite in iron only.

IV. The fourth conclusion is, that these ancient instruments contain none of the metals but copper and tin: for,

1. They do not contain *gold*, *silver*, or *platina*, excepting silver in the spear-head, as appears from the experiment of cupellation, § 4. (*e*).

2. They do not contain lead, for that would have oozed out in the experiments of fusion and oxidation; and would have appeared in the grain of the fractures; as well as on adding muriate of soda, and muriatic acid, to the concentrated nitrate solution, § 5. (*d*).

3. They do not contain *iron*, for that would have been shown by the prussiate of soda, § 5. (*d*); as was proved by the synthetical experiment, § 6. *Exper.* 17. (*b*).

4. They do not contain *zinc*, for that would have been shown by the blue flame and white flowers in *Exper.* § 4. (*c*) (*d*); as well as by the yellow colour of the grain of the fracture, which was shown by the synthetical experiments, § 6. *Exper.* 13, and 14.

5. *Bismuth* would have appeared on diluting the nitrate solution, § 5. (*d*).

6. *Manganese* would have been seen on concentrating by evaporation the nitrate solution, § 5. (*c*) (*d*).

7. *Arsenic* would have manifested itself by the brittleness and whiteness of the metals; by the smell and visible vapour on exposure to fire and air; and on examining the solution, § 5. (*d*), and the white deposit, § 5. (*e*).

8. *Antimony* would have produced more brittleness than these ancient metals possessed: a white vapour would have appeared on examining the white sediment with the blow-pipe, § 5. (*e*): as well as in the experiments in the assay furnace, § 4. (*b*) (*e*); and a white precipitate would have fallen on diluting the muriatic solution of the white deposit from the nitrate solution, § 5. (*e*).

9. *Cobalt* would have been detected by the prussiate of soda; and by the colour of the oxide, in the experiment in the assay furnace, § 4. (*b*); and it would have given brittleness to the ancient metal instruments.

10. It is not at all probable that *nickel* was present; but if it had been an ingredient, it most likely would have been betrayed by its greenish oxide in the experiment, § 4. (*b*).

11. *Molybdæna*, and *quicksilver* may be mentioned for the sake of order, but it is utterly unreasonable to suppose them to be present, either naturally or by art; and evident appearances, or at least traces of them, must have occurred in the preceding experiments. As for the substances called *tungsten*, *uranite*, *menackanite*, and *titanite*,* we have not yet had sufficient evidence to prove their being peculiar metals; but from the properties which have been observed to belong to them, it is quite inconsistent with the preceding experiments and obser-

* A new metal, named *Titanium*, lately announced in the German Journals.

vations to suppose them to exist in the ancient metal instruments. It will be proper also to remark, that the only species of metals known till within the last two or three centuries, were gold, silver, quicksilver, iron, copper, lead, and tin. The oxides of several of the brittle metals were known indeed to the Hebrews, Greeks, and Romans, and perhaps to several barbarous nations of great antiquity; but not one of them was used as an allay, except the oxide of zinc to compose artificial orichalcum.

It appears that the metal of the spear-head contained silver; but although the presence of it was proved by a repeated decisive experiment, § 4. (*e*), the proportion of it was too small to alter sensibly the properties of the allay of copper with tin, and could not answer any useful purpose in such a compound. I therefore believe that the silver in this instance was not purposely added; but was an accidental or natural ingredient of the copper, used for the making the metal of this spear-head. The Bishop of LLANDAFF made a few experiments on a celt, from which his lordship concludes that it seemed to contain zinc: for it emitted a blue flame, and a thick white smoke, on the first exposure of a piece to fire; but no such appearances were seen on the second exposure of the same piece to fire. Every person will readily give credit for the observations being accurately made; nor would I even refuse to admit the conclusion, that the celt examined by his lordship did contain zinc; but it is also just to observe, that a piece of copper, or of allay of copper, with tin, being exposed to fire in an open vessel, emits frequently a blue flame on a first, but not on a second exposure to fire soon after the first, § 4. (*c*); and if much air be admitted to the allay of copper with tin in fusion, a white smoke will also

sometimes be seen; as was observed in the preceding experiment, § 4. (*e*).

I suspect that the blue flame from copper when first ignited, and which ceases on fusion, is produced by the inflammation of a little of the copper already combined with oxygen; for some oxides of copper are so combustible, that if a small part of a given mass of them be ignited, the ignition will spread rapidly throughout the whole mass. Most probably celts were originally chopping tools, as we have shown in a former part of this paper, and therefore the addition of zinc to the allay of copper with tin would answer no useful purpose.

v. The fifth conclusion relates to *the proportion of the copper and the tin* to each other, in the ancient metals. I endeavoured to estimate the proportion of tin, by comparing the quantities of oxide of tin obtained from the ancient metals, with the quantities of oxide of tin obtained by the same means from allays of copper with known proportions of tin.

It appears from the analysis of the allays of copper by tin, that the oxide of tin afforded by the nitric acid solution is in the proportion of about 150 parts from every 100 parts of the metal tin, § 6. *Exper.* 1st—9th. According to this *datum* the proportion of tin in the old metals is in the following proportions, or nearly so.

1. The sauce-pan contains of tin a little more than 14 *per cent.*; that is about one part of tin and six of copper.

2. The spear-head contains 14 *per cent.* of tin; that is, somewhat less than one part of tin and six of copper.

3. The celt, No. 3; a little more than 12 *per cent.* of tin; that is, about one part of tin, and seven and a half parts of copper.

4. The lituus; nearly the same proportions of tin and copper as the celt, No. 3.

5. The scabbard; a little more than 10 *per cent.* of tin; that is, about one of tin and nine parts of copper.

6. The celt, No. 1;* a little more of tin than 9 *per cent.*; that is, about one of tin and ten parts of copper.

7. The celt, No. 2; the same proportions of tin and copper, as in the celt, No. 1.

VI. The two last conclusions are confirmed by the exact correspondence, between the ancient metals and the allays of copper by tin, in external and obvious properties, § 2. and § 6; in specific gravities, § 3. and p. 422, l. 21; and in chemical properties, § 5. and § 6. Allays of five to eighteen parts of copper with one part of tin can generally be distinguished from such allays with the addition of a very small proportion of the other metals; by the colour of their polish, the colour and texture of their grain, their strength, their hardness, their malleability, and specific gravities; without the aid of chemical analysis. It is worthy of remark, that these allays of copper with tin are evidently different, in their colour and grain, from such allays with the addition of even one-fortieth of their weight of zinc, *Exper.* 13th; and also from copper allayed by one-fortieth of its weight of zinc, *Exper.* 14th.

The similarity of the properties of the ancient metals, and of the allays of six to twelve parts of copper with one of tin, is very evident. But with smaller proportions of tin we find the allays are softer, and the grain of their fractures more open

* Mr. Cavallo made a few experiments on a very small quantity of this celt before it came into my possession; from which he conjectured that it consisted of one part of tin, and six parts of copper.

than the ancient metals, *Exper.* 1—4.; and with larger proportions of tin we find the allays harder, more brittle, paler, and closer in texture than the ancient metals, *Exper.* 8.—11.

It is right, however, to remark, that the property of hardness of the allays of copper by tin is, *cæt. par.* as the proportion of tin, or nearly so; which is not the case with some of the ancient metals; for the spear-head and sauce-pan contain rather more tin than an equal quantity of the lituus, § 5. (*c*), which is much harder than them, § 2. (*c*); and the spear-head and sauce-pan are nearly as soft as the celts, No. 1 and No. 2, § 2. (*c*), which contain the smallest proportion of tin of any of the old metals, § 5. (*c*).

The grain also of the fractures of the spear-head and sauce-pan, before melting, is much coarser, or open, than those of the other ancient metals which contain a smaller proportion of tin, § 2. (*b*), p. 404, l. 21: but it appears from the synthetic experiments that the grain becomes finer as the proportion of tin is increased, § 6. *Exper.* 1.—12. To account for these inconsistencies I must remark, that a minute quantity of extraneous unmetallic matter may be contained in metals; so minute indeed as to elude the most rigorous analysis, or at least not to be discoverable by the ordinary modes of examination; and which also may not render the metal at all unfit for most of the uses to which it is applied. For instance, good malleable iron may contain carbon, and even phosphate of iron or siderite; and metals in general may contain a very small proportion of oxygen, and yet be as useful as the purest metals. The best English copper is accounted less tough and ductile than Swedish copper. The purest English tin crackles when it is bent or chewed, but pure Malacca tin has not this property. These

differences of properties most probably depend upon some extraneous matter; but in so small a proportion as to have hitherto eluded the research of analysis.

In the case before us, it is probable that a very minute proportion of extraneous matter was present in the spear-head and sauce-pan, especially as they were made of cast metal; which might be less hard, and less compact in texture, than an allay of pure metals containing a smaller proportion of tin to the copper, and yet the allay might be less brittle than the cast metal. This extraneous matter may be oxygen, or sulphur, or earth, although in an imperceptible quantity, introduced during the fusion. The lituus is harder, and not more brittle than the spear-head and sauce-pan; although it contains less tin. It was made of a plate of metal which had been much hammered, and must therefore either originally have been made of purer metal than the spear-head and sauce-pan, or have been rendered purer by hammering. Perhaps metals in general are rendered purer, more uniform in texture, and more dense, by remelting, than they were immediately after casting from the ore; or in the case of steel immediately after cementation; or in the case of allays after the fusion by which the union was effected. Accordingly, cast iron is rendered less brittle by repeated fusion; Mr. HUNTSMAN's cast steel is made by merely remelting steel which had been manufactured by cementation; and Mr. MUDGE's speculum metal, an allay of copper by tin, was not uniform and sufficiently compact till it was remelted. The specific gravity of the sauce-pan, and spear-head, was particularly increased by fusion, § 3. 2. 3; and their texture was rendered more uniform and compact, § 2.

The specific gravities of the ancient metals correspond, as

nearly as should be expected, with their composition found by analysis; and agree sufficiently with the synthetic experiments, § 3. and § 6. p. 422.

I did not find that the specific gravity of the same metal, under known circumstances alike, was so nearly the same in all cases as is stated by most writers. In the preceding experiments, different parts of the same ingot varied more than is commonly supposed in point of specific gravity.

The specific gravities of the ancient metals, after melting, varied between 8,5 and 8,8, or nearly so, § 3; and the specific gravities of the allays of three to twenty parts of copper with one of tin varied between about 8,5 and 8,9, § 6. p. 422. These great specific gravities seem surprising, because that of tin is only about 7,2, and of copper ingot about 8,420, § 6. p. 422. But of all metallic combinations that of copper with tin produces perhaps the greatest increase of density. ARISTOTLE made this observation long ago,* and the fact is familiarly known to manufacturers of bell-metal. But it does not appear that the increase of specific gravity is so great as it is stated by GLAUBER. According to him, if two balls of copper and two balls of tin of the same dimensions be melted together, the compound will afford scarcely three balls of the same dimensions as each of the four balls; and yet the three balls will weigh as much as the four balls.—" Funde prædictos globulos in unum ..... " iterum effunde mixturam liquefactam in typum globulorum

* Ταῦτα γαρ μείζω δὴ και πλείω ποιεῖ μόνον τὸν ὄγκον. Ὅταν δ' ἢ θάτερον μόνον παθητικὸν, ἢ σφόδρα, τὸ δέ πάμπαν ἠρέμα, ἢ ὐδὲν πλεῖον τὸ μιχθὲν εἶναι ἀμφοῖν, ὅπερ συμβαίνει περι τὸν κατ͂ίτερον και τὸ χαλκόν.————————Ὁ γαρ κατ͂ίτερος ὡς πάθος τι ὤν, ἄνευ ὕλης τοῦ χαλκου, σχεδὸν αφανίζεται, και μιχθεὶς ἅπεισι, χρωματίσας μόνον.—ARISTOTLE, ΠΕΡΙ ΓΕΝΕΣΕΩΣ ΚΑΙ ΦΘΟΡΑΣ ΤΟ Α. Κεφ. ι. Ὅτι ἔστι μὶξις.

" primorum, et non prodibunt quatuor sed vix tres numero " globuli, pondere quatuor globulorum reservato."—GLAUBER, *de Furnis, pars* iv. p. 67. 8vo. 1651.

The specific gravity of the allays of copper by tin, and the following experiment, show that the contraction in the dimensions of these two metals on combination, cannot be so great as stated by GLAUBER.

I made two ingots of tin and two of copper, of nearly the same figure and dimensions. The specific gravity of the tin was 7,233, and that of the copper was 8,594. The absolute weight of these four ingots was 1730 grains. On combination by fusion, the compound afforded three ingots, and one-third of an ingot, of the same dimensions as the original ones; but those weighed only 1640 grains; 90 grains being wasted and adhered to the melting pot. The specific gravity of one ingot of this metallic combination was 8,340; and of another, 8,4. Consequently, after making the most reasonable allowance for the errors of the experiment, the contraction could not be one-fourth of the sum of the bulks of the metals previously to fusion, according to GLAUBER; but it might be about one-eighth.

VII. I next observe, that the proportions of tin found in the ancient metals consist with the uses for which they were made.

The principal uses of the allay of copper by tin are, to render copper less oxidable by water, or atmospheric air; to give hardness; to render it sonorous; to render it more fusible; to produce a close texture and whiteness for reflecting light; and to render copper less tough and clingy.*

Copper allayed with one of the smaller proportions of tin by manufacturers, is metal of which guns or cannon, improperly called *brass guns*, are made. Different proportions

* The workmen say, *claggy*.

of these two metals are used at different manufactories; but I believe that this gun metal seldom contains less than one part of tin to twelve of copper, nor more than one part of tin to nine of copper. Here as much strength, as is consistent with the preservation of the figure of the instrument during its use, is required: and if more tin were added, the gun would be liable to be fractured by the explosion; and if less were added, it would be liable to be bent.

Copper allayed with a somewhat larger proportion of tin than in gun metal in general, affords a metal sufficiently hard and strong for chopping tools, for many useful purposes. Of such proportions, namely, about eight or nine parts of copper and one part of tin, there is very little doubt all the ancient nations, who were acquainted with the allays of copper by tin, generally made their axes, hatchets, spades, chizzels, anvils, hammers, &c. These metals, united in these proportions, would, I believe, afford the best substitute known at this day for the instruments just mentioned, now commonly made of steel or iron. Accordingly, before the art of manufacturing malleable iron from cast iron was known at all, or at least practised extensively, that is, till within these last four or five hundred years, the allays of copper by tin must have been very generally employed. The celts may be considered as specimens of the kind of metal tools in general use before the art of manufacturing iron in the manner just mentioned was discovered: for, as hath been remarked in a former part of this paper, the celts seem to have been generally neither more nor less than metal heads of hatchets, and axes, or other chopping tools. And it is no small confirmation of this opinion, that by analysis and synthesis we have found those metals to contain, in perhaps most instances, the proportion of tin which renders them most fit for the uses to

which they were applied. This proportion being considered to be about one part of tin to nine parts of copper.

Copper allayed with a larger proportion of tin than is generally contained in celt metal; that is, with one-sixth or one-seventh of its weight of tin, is fitter for cutting instruments, and piercing, boring, and drilling tools than celt metal; because it is harder, takes a finer edge, and yet is sufficiently strong on most occasions; nor do we possess at this day any metal, that I know, which is so fit for knives, swords, daggers, spears, drills, &c. as this allay, except iron and steel.

The spear-head contains tin in the very proportion here mentioned; and if the metals had been pure, it would not have been possible, perhaps, to have made this instrument of any other metals, which were so proper, and at so small an expence.

The sauce-pan also was made of allay of copper by tin in the proportions last mentioned, § 7. p. 428; but as this instrument is sufficiently hard with less or without any tin, there seems to be no use from the addition of it. We may conjecture, indeed, that as the sauce-pan was made of cast metal, the tin was added for the purpose of rendering the copper more fusible, and thereby also for more easily casting forms of it. Perhaps also the tin was added to render the copper less readily oxidable, and for the colour of this composition.

Copper united with the proportions of tin last mentioned is very sonorous; but it is rendered much more so by still larger proportions of tin. I apprehend the sonorous property increases as the proportion of tin is increased, within certain limits; provided the allay possess sufficient strength not to be fractured by the necessary impulse. But as the brittleness increases with the increased proportion of tin, I believe not

more than one part of tin is added to three parts of copper to compose the most sonorous metal which is manufactured, namely, bell-metal.* But this allay is too brittle to be beat out into a plate for making a trumpet; and accordingly the lituus, which has been made of hammered metal, contains only about one part of tin and seven parts and a half of copper, § 7. p. 429.

Copper is united with tin for the purpose merely of becoming more fusible, and of continuing longer fluid, or cooling more slowly while passing from the melted, or fluid state, to the solid state. Such metal is used for making statues, and casts of figures in general, and is called statuary metal,† and sometimes bronze. The proportions of the two metals are various; probably according to the colour proposed, and the size and figure of the cast; as well as on account of the price of the metals.

A small proportion of zinc is sometimes added to allays of copper by tin; on some occasions, on account of colour, on others, perhaps, to render the copper still less oxidable and more fusible; and on other occasions, as I have found on inquiry, it is added from erroneous theory, or mere caprice. No one could tell me the use of zinc, which in some manufactories is added, in making gun metal.

Tin might be used also to render copper less clingy, or

* The proportion of tin varies in bell-metal from one-third to one-fifth of the weight of the copper; according to the sound required, the size of the bell, and the impulse to be given.

† The Greeks and Romans consumed vast quantities of copper in casts of figures. They added not only tin but lead to the copper. The proportions given by PLINY are one part of a mixture of equal quantities of lead and tin to fifteen parts of copper. The use of the lead I do not understand, if it was not to save expence.

more brittle, for the purpose of writing upon it, or marking it with lines and figures, as on mathematical instruments: but the allay with zinc is now preferred for these purposes, as I suppose on account of its being less hard than allays with tin, and yet sufficiently brittle; on account also of its golden colour; and also on account of its being still more difficultly oxidable by air and water.

The scabbard metal contained a rather larger proportion of tin than the celts, No. 1. and No. 2.; namely, being one-tenth of its weight, § 7. p. 429. Copper allayed by zinc would have been sufficiently hard and strong, and on other accounts preferable to the allay of copper with tin. This is, however, one proof of the extensive use of this last composition among the ancients.

The art of allaying copper with an earth-like substance; which, within a little more than these fifty years only, we have learned was an ore of a metal, namely, zinc; was known perhaps in the time of ARISTOTLE, and certainly of PLINY; for the latter informs us, that this composition *resembles orichalcum;* and *after his time* it was called orichalcum. Thus the *native* and *factitious* orichalcum were confounded. The ancients do not appear to have used the allay of copper by zinc, except for mere ornaments, to resemble gold. It is much more extensively employed by the moderns, and the allay of copper with tin is less extensively used: 1st, because the former is cheaper than the allay of copper with tin; 2dly, because it is now generally understood that it preserves its colour longer; 3dly, because it is easier to work it into various forms, and especially for philosophical instruments; few of which were probably made by the ancients.

The composition in common use, which contains the greatest proportion of tin, is called speculum metal. The requisites of this metal are compactness, uniformity of texture, whiteness, sufficient strength to prevent its cracking in cooling, and to bear polishing without breaking. Mr. MUDGE found the whole of these properties attainable in the greatest degree, by a little less than one part of tin with two parts of copper. But for very large instruments; such as the 40-feet telescope of Dr. HERSCHEL; the proportion of tin must be less than in small instruments, on account of the property of brittleness.

The compound of equal weights of copper and tin is so brittle, that it is not easy to conceive to what useful purpose it can be applied.

The allays of tin with copper, by which I mean those compounds of copper and tin in which the tin is in greater quantity than the copper, I believe, have not been examined. It is said, indeed, that tin allayed with a very small proportion of copper has been employed for tinning, to save much of the expence of tin; for a much thinner coat of this compound can be spread than of tin.

VIII. The next conclusion is founded on the experiments of the allays of copper with steel.

It appears that copper may be united to steel without the intermede of any other metal; for a perfectly homogeneous compound was produced by melting ten parts of copper with one of steel, § 6. *Exper.* 19. As this allay was not harder than that of copper with one-twentieth of its weight of tin, and as it did not appear that a compact and uniform malleable metal could be composed of one part of steel with two partsof copper, § 6. *Exper.* 20. I thought it unnecessary to make any

more experiments with different proportions of copper and steel. For, 1st, granting that the allays of copper by steel are as hard, strong, and malleable as those of copper by tin, it is utterly improbable that the ancients should have used steel to harden copper; on account of the great scarcity and high price of steel comparatively with tin; and also on account of the difficulty of uniting copper with steel, but facility of uniting copper with tin.

2dly. It appears that no allays of copper by steel can be made, which possess the hardness, strength, and malleability required; but which required properties we obtain by combinations of copper with tin, and with which most indubitably the ancients were well acquainted. Count CAYLUS has indeed told us, that the ancients had two methods of hardening copper; namely, by cementation, and by allaying it with iron. The first method he has not explained; nor is any method known of hardening copper without addition, except by hammering it; which it is well understood cannot produce the required hardness. As to the other method by allaying with iron, I think myself warranted in refusing the Count's single vague evidence; and in admitting the evidence of other plainly decisive experiments; which consist also with reasoning and analogy.

Philological, and antiquarian writers in giving an account of the copper arms and utensils of the ancients: as they found them much harder than copper, and that they were used for purposes to which copper would have been quite unfit; and as they saw that the ancients commonly used copper on most of those occasions in which we now use iron, or steel; were led to imagine, that in ancient times there was an art understood

of tempering copper, which had been subsequently lost.* If, instead of feigning such an hypothesis, these writers had examined by analysis the ancient implements which fell under their observation, I cannot doubt that they would have unravelled the mystery. Count CAYLUS himself had a glorious opportunity of ascertaining the composition of ancient copper instruments, when the seven swords, and hollow wheel were found at Genzac in 1751. If he had made but two adequate experiments, one to detect iron, and the other to detect tin, he would have had a much better foundation for reasoning than that of a mere hypothesis, however ingenious and learned.†

* "It appears, says Dr. LORT," in his paper upon celts, "that the ancients had "an art of tempering and hardening brass to a greater degree than is done at present, or perhaps than is necessary to be done." Archæol. Vol. V. p. 187.

With reluctance I must observe, that such an experienced inquirer as Dr. PRIESTLEY falls into the error of antiquaries, in asserting, that the ancients had a method, with which we are not well acquainted, of giving copper a considerable degree of hardness, so that a sword might be made of it with a pretty good edge. But PAUW tells us, that the Americans were in possession of the secret of giving a temper to copper equal to steel.

† At the desire of Count CAYLUS, the old metal instruments found at Genzac were examined by GEOFFROY, the younger. To prevent all suspicion of this celebrated chemist being defrauded of the honour due to him by mistaking his meaning, I shall cite his own words.

"Je cherchai à m'assurer s'il y avoit dans ces armes antiques une portion d'étain "sensible et aussi considérable que dans le métal que j'alliois. Pour cet effet je mis "dans un bain de plomb sur une coupelle un petit morceau de mon alliage, qui "aussi-tôt qu'il commença à se fondre, végéta, à cause de l'étain qu'il contenoit. J'ai "repété cette expérience, sur le métal des armes antiques, et ce métal n'ayant point vé-"gété, mais étant plus difficile à fondre que le mien, je fus convaincu que ce n'étoit "point l'étain qui durcissoit le cuivre, qui est le métal principal des armes."

This was the experiment for detecting tin; and the following is the account of the methods employed to determine the presence of iron.

There is not the least reason to suppose that the ancients added iron or steel to increase the hardness, or strength of the allay of copper by tin; nor does it appear from the experi-

" La difficulté que j'avois trouvée à fondre ce métal, me fit soupçonner qu'il conte-
" noit du fer, et mon soupçon se changea presqu'en certitude lorsque je comparai le
" grain de ce métal avec celui de quelques essais de cuivre alliés de fer, que mon père
" avoit fait dans le temps qu'il donna à l'Académie des Sciences un mémoire sur le
" tombac. Pour m'assurer s'il y avoit du fer dans ce métal j'en reduisis un petit mor-
" ceau en limaille, et la plus grande partie de cette limaille étoit attirable à l'aimant;
" mais il me restoit encore un doute sur cette preuve de l'existence du fer dans les
" armes anciennes. *Le métal dont elles sont faites étant plus dur que l'acier,* eût usé
" quelques petites parties de la lime par le frottement; ce que n'auroit point fait le
" cuivre rouge qui est beaucoup plus tendre. Ainsi pour avoir une preuve qui ne
" laissât aucun scruple, j'ai fait user à cette même meule avivée un morceau de métal
" des armes anciennes, et en lavant la boue qu'il m'a fourni pour séparer les parties
" terreuses de la meule des parties métalliques, j'ai eu une espèce de limaille extrème-
" ment fine, qui étoit attirable à l'aimant; et je ne pouvois plus craindre alors de m'être
" trompé moi-même par trop peu de défiance. Etant bien convaincu que le cuivre
" des armes anciennes étoit allié avec du fer, j'ai fait préparer plusieurs essais d'un
" métal que j'ai cru à peu près pareil, en fondant du cuivre rouge, dans lequel je faisois
" entrer du fer dans différentes proportions, et tous ces essais m'ont fourni un métal
" durci; mais si aigre qu'il étoit impossible de le forger. Cet obstacle qui m'arrêtoit
" a été bientot levé lorsque en examinant ces armes avec plus de soin, et en consultant
" à ce sujet des ouvriers connoisseurs, j'ai été assuré qu'elles étoient jettées en moule, et
" ensuite réparées à la main. J'ai cherché à imiter pour la dureté et pour le tranchant
" une épée Romaine, et je crois n'y avoir pas mal réussi dans celle que j'ai remise à
" M. le Comte de CAYLUS. Elle est faite avec un mêlange de cinque parties de cuivre
" rouge et d'une partie de fer fondues ensemble, puis jettées en moule."

I cannot persuade myself that it is necessary to comment upon the above passages, which Count CAYLUS considers as showing *decisively* that the ancient metals were hardened by iron, and not by tin. As to this learned antiquary's other method of hardening copper, which he says was employed by the ancients, it is falsified by daily experience: for it is well known that this metal is not susceptible of temper like steel, either by plunging it in any cold medium whatever, or by cementing it with salts, and carbonaceous matter. See the *Recueil d'Antiquités Egyptiennes, Etrusques, Greques, et Romaines. Tome premier,* 4to, 1761.

ments with this mixture, *Exper.* 17, and 18, that any advantage is to be expected from this addition; at least not for cutting instruments.

I cannot confirm the opinion above delivered, that the common metal of the ancients for cutting instruments was the allay of copper with tin, by the experiments of other persons, excepting those of Mr. DIZE', in the *Journal de Physique* for 1790, p. 272. He had only twenty-five grains of an ancient dagger to operate upon. This small quantity, however, afforded tin and copper, as appeared on dissolution in nitric acid. But Mr. DIZE' made several analytical experiments on eight different sorts of coins, Greek, Roman, and Gallic. It appears from these experiments that these coins contained from $\frac{5}{12}$ of a grain to $24\frac{1}{3}$ grains of tin in 100 grains of each of the old metals. And it appears that these coins contained no other metals but copper and tin.

From the preceding experiments and observations we learn that tin was infinitely more valuable to the ancients than it is to the moderns: without this metal, it is not easy to conceive how they could have carried on the practice, and invented the greater part of the useful arts. Tin was even of more importance to the ancients, than steel and iron are to the moderns; because allays of copper by tin would afford better substitutes for steel and iron, than any substitutes which the ancients, in all probability, could procure for allays of copper by tin.

We see also the importance of Britain in times more remote, probably, than those of which we have any record, or tradition; being, in all probability, the only country which furnished the metal so necessary to the progress of civilization. If Mr.

LOCKE had been acquainted with the properties of the allays of copper by tin, and of their extensive use in highly advanced states of civilization among the ancients; he would have known that iron was not the only metal by the use of which we are in possession of the useful arts, nor consequently is it "past "doubt, that were the use of iron lost among us, we should in a "few ages be unavoidably reduced to the wants and ignorance "of the ancient savage Americans." In the barbarous state of its inhabitants, this island was known to the civilized nations of Europe, Asia, and Africa; and denominated in two of the most ancient languages, namely, the Phœnician and Greek, by terms which denote, *the land of tin;* for such, according to BOCHART, is the import of Britain, a corruption of *Barat-Anac*, or *Bratanac;* and there is no doubt of the meaning of the Greek word *Cassiterides*.

I do not mean by these observations to represent, as authors in general have done, that the ancients were not acquainted with the art of manufacturing iron, or steel, till long after the common use of copper, or that they did not know the superior properties of iron and steel; on the contrary, if this were the proper place, I could show that iron, or at least steel, was manufactured, and its useful properties understood, as early as copper was known. But steel was got anciently from those ores only which yield it in a malleable state; as it is, probably, obtained at this day in India, and called *wootz;* and as it is also obtained in the northern Circars, and likewise by the Hottentots. As steel was the only state of iron anciently manufactured, it was too scarce, and much too dear for general use; and hence the extensive use of allays of copper by tin, the best substitutes for the malleable state of iron and steel.

SECTION II. OF THE STEEL ARMS.

§ 1. *A few miscellaneous Observations.*

Of the ancient steel or iron arms and utensils in Sir JOSEPH BANKS's collection, four articles only were selected for examination.

One of these was the Steel Sword within the copper scabbard, described in Sect. I. § 1. IV. and represented by fig. 3. Tab. XI.

1. *A Sword*, fig. 1. Tab. XV. Of a number of these weapons in the collection this was the smallest. The great difference in their size and weight, it is observed, was probably intended to give every man, according to his strength and mode of fighting, an opportunity of suiting himself. The figure of the blade is particular, and seems very well contrived.

The hollow in the middle of each side does not extend more than two-thirds from the guard to the point; and terminates in a ridge, which must give great support and strength to the cutting part.

The pommel and guard had been tinned, and part of the tin coating still remains upon them. This weapon, therefore, affords a specimen of the mode of tinning iron practised by the ancients. The blade seems to have been varnished by black matter, which remains very brilliant and smooth. On one side is the inscription + BENVENUTUS +, and on the other + ME FECIT +, perfectly legible.

From the crosses, we may conclude that the maker was a Christian; and from the name, that he was an Italian. The writing is in mixed characters, but it is probable that the artist exercised his trade of a sword cutler in the northern

parts of Europe. We cannot however determine whether it be Danish, or Saxon. It was found in the river Witham, with a large quantity of other arms, in the neighbourhood of the site of Bardney abbey; and was brought up by an eel-spear, by a man who was fishing in that river, near Kirksted Wath, in 1788.

II. *An Axe.* Its form is evident from the fig. 2. Tab. XV. It was snipt a good deal, and several holes were worn in the middle, otherwise it was in a state of good preservation. It was found, with other axes, chopping instruments, and carpenters' tools, in the river Witham, in 1787, and 1788.

This axe perfectly resembles that carried by the lictors in their fasces, in basso-relievos. Its form induces one to suppose, or indeed to believe, that it was made for parade rather than use; its edge being very thin, and immediately above it the blade being thicker; but behind the thick part, exactly where the strength of an axe ought to be placed, it is thinner than in any other part. It was therefore not well calculated for chopping. The weight of this axe was one pound, and somewhat more than a quarter. Its length from eye to edge was seven inches, and the breadth was about six inches.

III. *A Dagger:* its form is represented by fig. 4. Tab. XV. It was made with great ingenuity and skill for answering the main purpose of it, that of piercing armour. It was found, together with another dagger, in Barling's Eau, near Short Ferry, in 1788.

IV. *A Sword in its Scabbard*, fig. 3. Tab. XI. I could not by any force draw it out of the scabbard. On breaking the scabbard, I found the sword destroyed by rust; but the guard, and hilt were still in a metallic state, and the pommel had

been broken off. I have already described this instrument in the account of the brass arms.

§ 2. *Chemical Properties.*

1. *The Sword*, fig. 1. Tab. XV.

(*a*) Being filed and polished, it was of the colour of steel.

The blade was bent considerably before it was broken; and could not be broken without considerable force.—Comparatively with soft steel, or malleable iron, it possessed little malleability.—Under the hammer, file, and drill, it felt as hard as hardened steel. The snipt edges were hard, and strong enough to saw asunder the celts, described in this paper. Its fractured surfaces showed a silvery kind of open grain, like steel which has been hardened by plunging it, when white hot, in cold water.

The pommel and guard were much more malleable, and much less hard than the blade.

(*b*) The blade, when red hot, was malleable, but much less so than our common steel. On cooling gradually it became less hard than before; but it was not so soft as our common annealed, or distempered steel. By plunging the distempered piece of the blade, when white hot, in cold water, it was restored to its original hardness. By plunging the pommel and guard when white hot in cold water, they were rendered much harder; and by again igniting them, and letting them part with their fire gradually, they became as soft as they were originally.

(*c*) The specific gravity of the middle part of the blade, after filing off the coating, was 7,476.

(*d*) The dissolution of 300 grains of the blade in sulphuric acid and water, yielded nearly the same quantity of hydrogen

gaz as an equal quantity of our steel affords. During the dissolution the mixture became black, and a black froth appeared on its surface; and, after repose, there was a deposit of black matter. The solution, made boiling hot, was poured upon a paper filter; and being filtrated, the filter was edulcorated, by repeatedly pouring upon it pure water. The paper filter was stained black by the solution, and there was a small deposit of black matter in the apex of the cone of the filter. This black matter was carbon, in about the same proportion as our steel affords by the same treatment. The filtrated solution, on evaporation, was found to contain nothing but sulphate of iron.

(*e*) A little nitric acid being dropped upon the polished surface of the blade; and also on the pommel and guard; a black spot was produced on the parts wetted.

(*f*) The tinned part of the pommel being just wetted with nitric acid, it became white.

11. *The Axe*, fig. 2. Tab. XV. (*a*) Being polished, it appeared of almost a silvery whiteness.—It was harder than malleable iron, but was not so hard as hard steel, for it was easily filed, and bored through with the drill. It was also cut through; and the cut surface was smooth and uniform, and close, as if made of the purest metal. It bent a little, notwithstanding its form and thickness; and required a very smart stroke with a heavy hammer to break it. The grain of the fractured part was like that of close-grained steel.—It was malleable both in its cold and ignited state.—It was almost as sonorous as bell-metal.

(*b*) By quenching in cold water when ignited to whiteness, it became harder, more brittle, and open-grained; but it could not be made so hard as the sword, fig. 1. By igniting the piece so hardened, and letting it part with its fire gradually, it was rendered much less hard than it was originally.

The artist who assisted me in examining this tool, observed that it was only made of steel for about an inch from the edge; but that the rest was iron; for he conceived it to be impossible to be all steel, on account of the eye for the wooden shaft. However, on filing different parts, and cutting the instrument, no seam could be discovered, where iron had been welded to steel; and every part appeared susceptible of induration and emollition, by the usual treatment of steel to produce these changes.

(*c*) The specific gravity, before hammering, was 7,802, and after hammering the same piece, it was 7,880. After ignition to whiteness and sudden quenching, the specific gravity was 7,384.

(*d*) 300 grains of this metal dissolved in sulphuric acid and water, and afforded black matter and sulphate of iron; with the same phænomena as the dissolution of the sword, fig. 1. afforded. The black matter was carbon, in apparently the same proportion as was obtained from the dissolution of the sword, fig. 1.

(*e*) Several parts of this axe being just wetted with nitric acid, they became black spots, as is the case on so applying this acid to steel.

III. *The Dagger*, fig. 4. Tab. XV. (*a*) Being polished, it had the appearance of steel.—It was not so hard as the sword, fig. 1. but it was so very strong and tough, that it was with difficulty broken, and could be bent very considerably.—Its fractured, or rather torn surface was open-grained, and crystallized.—It was more malleable when cold, than hardened steel usually is.

(*b*) In its ignited state it was very malleable. It was susceptible of induration and emollition, by the ordinary treatment to produce these changes in steel.

(*c*) The specific gravity of this dagger was 7,413.

(*d*) The dissolution in sulphuric acid and water afforded

nothing but carbon and sulphate of iron, in about the same proportions as the dissolution of the sword, fig. 1.

(*e*) A black spot was produced by wetting this instrument with nitric acid.

IV. *The Sword*, fig. 3. Tab. XI. (*a*) The hilt being polished, it appeared like steel. It was almost as hard as common hardened steel, and as malleable.

(*b*) The hilt was very malleable in its ignited state. It was hardened, but not considerably, by quenching in cold water when white hot. It was rendered softer, after being hardened, by ignition and gradually cooling.

(*c*) The specific gravity of the hilt was 7,647: and after ignition and quenching, it was 7,427.

(*d*) The nitric acid produced black spots when applied to the polished surfaces of this metal.

(*e*) The dissolution in sulphuric acid and water afforded nothing but carbon, and sulphate of iron. The carbon was in smaller quantity from this hilt, than from the sword, fig. 1. and not more than from common steel.

§ 3. *Conclusions and Remarks.*

1. It appears that all these instruments are of steel; because they consist of carbon and iron, § 2. I. (*d*) (*e*); II. (*d*) (*e*); III. (*d*) (*e*); IV. (*d*) (*e*); because they are capable of induration by plunging them when ignited in a cold medium, § 2. I. (*b*); II. (*b*); III. (*b*); IV. (*b*); and they are softened by ignition and gradual cooling *(ibid.)*—they have the colour, texture, hardness, brittleness, § 2. I. (*a*); II. (*a*); III. (*a*); IV. (*a*); malleability when ignited, § 2. I. (*b*); II. (*a*) (*b*); III. (*b*); IV. (*b*); and specific gravity of many sorts of steel, § 2. I. (*c*); II. (*c*); III. (*c*); IV. (*c*).

2. The sword, fig. 1. appears to be the hardest, § 2. I. (*a*), and the dagger, fig. 4. the softest steel of the above instruments, § 2. III. (*a*).

3. These steel instruments appear to have been tempered, at least in the parts destined for cutting, and piercing.

4. The axe, fig. 2. being all steel, affords a proof that the ancients were not acquainted with the art of manufacturing soft malleable iron; nor consequently of welding it with steel; and that the only state of iron which they used, and could manufacture, was steel.

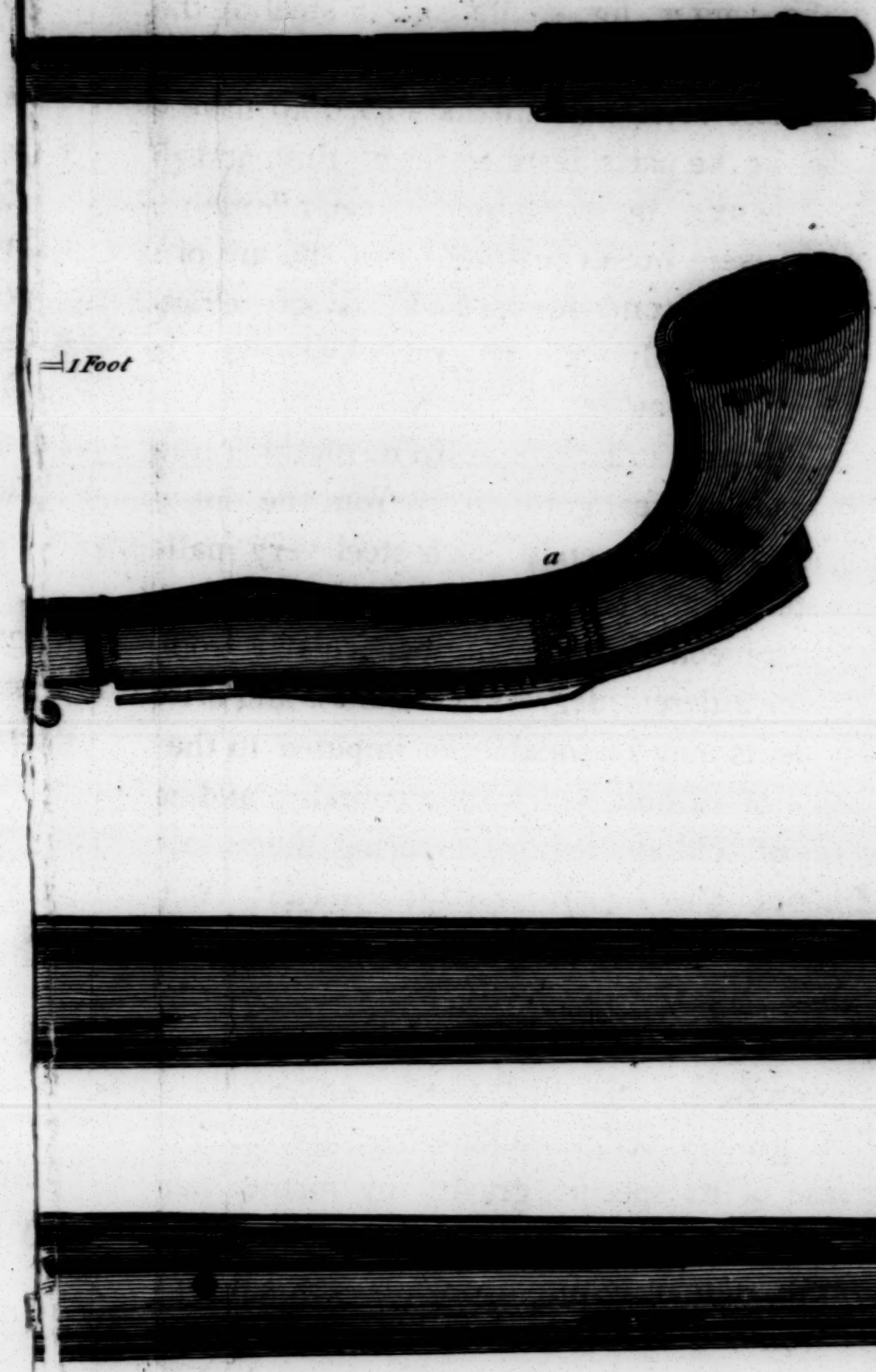
5. Although, it is most probable, that these steel instruments were made of steel got directly from the ore, they show that the ancients could render such steel very malleable in its ignited state, § 2. I. (*b*); II. (*a*) (*b*); III. (*b*); IV. (*b*); and free from extraneous matters, and particularly from oxygen.

6. The different degrees of hardness and brittleness of these instruments may reasonably be imputed to the different proportions of carbon which they contain; and to the different degrees of cold applied in tempering them; although the experiments, § 2. I. (*d*); II. (*d*); III. (*d*); IV. (*e*), were not made with such precision as to demonstrate the reality of these assigned causes.

7. It seems probable that the axe was tempered at a low temperature, and had been much hammered: hence its great specific gravity before hammering, § 2. II. (*c*), and the little increase of its specific gravity by further hammering; and hence the great diminution of its specific gravity by quenching in its state of ignition to whiteness, § 2. II. (*c*).

8. Iron and steel instruments are destroyed, commonly, by the oxygen of water, or oxygen of atmospherical air. The destruction of iron instruments is prevented by whatever prevents

Fig. 1.



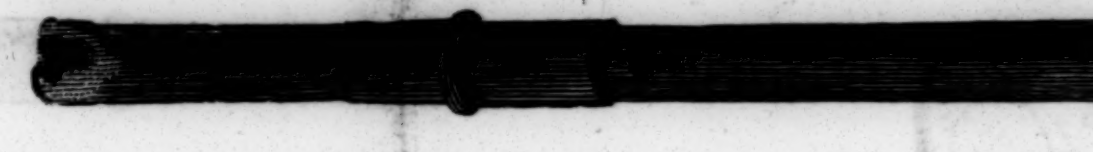
1 Foot

2 Feet

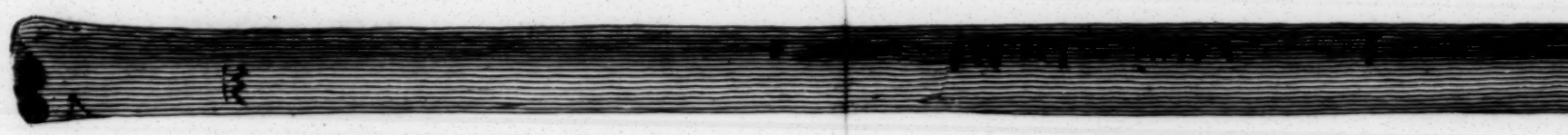
Bajou d'c.

Fig. 2.





Scale 6 *Inch.*



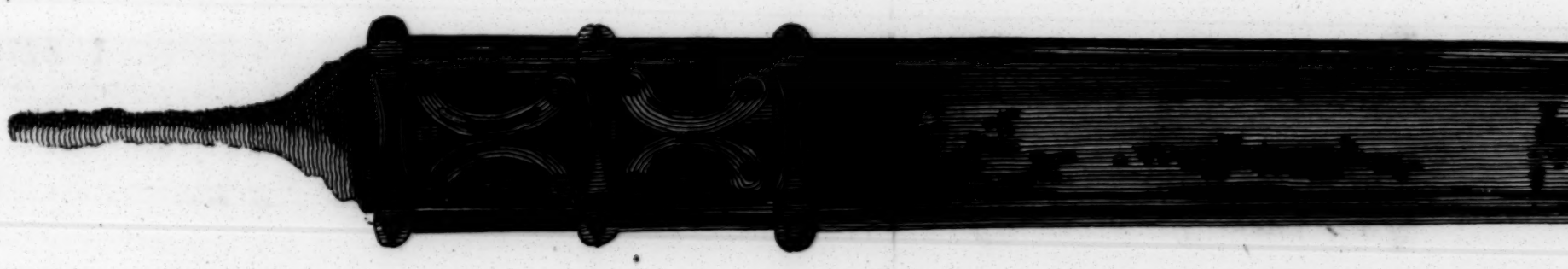
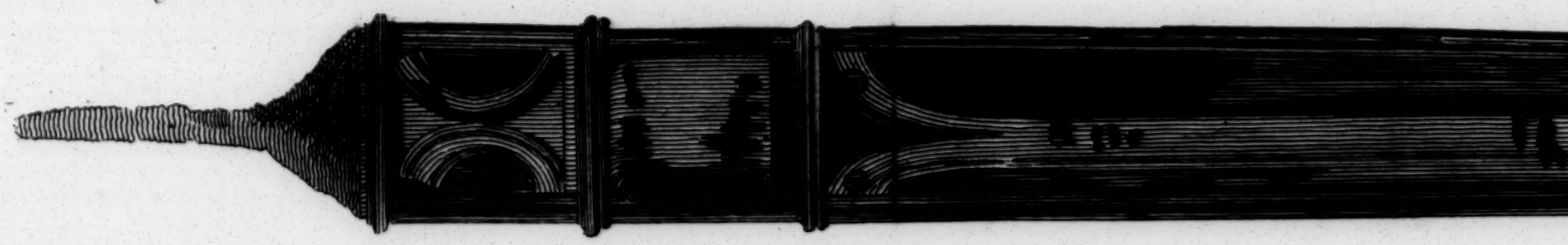
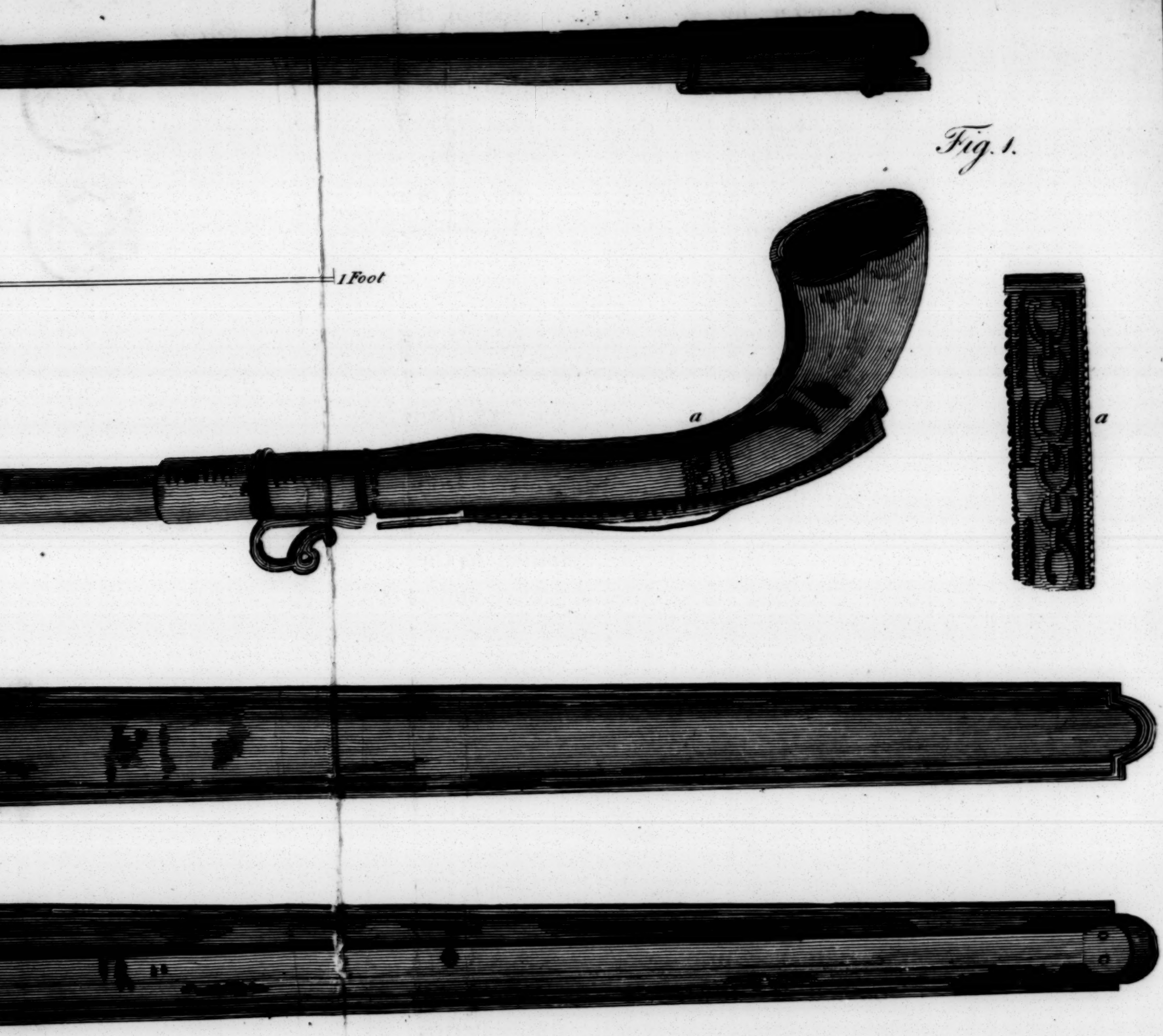


Fig. 3.

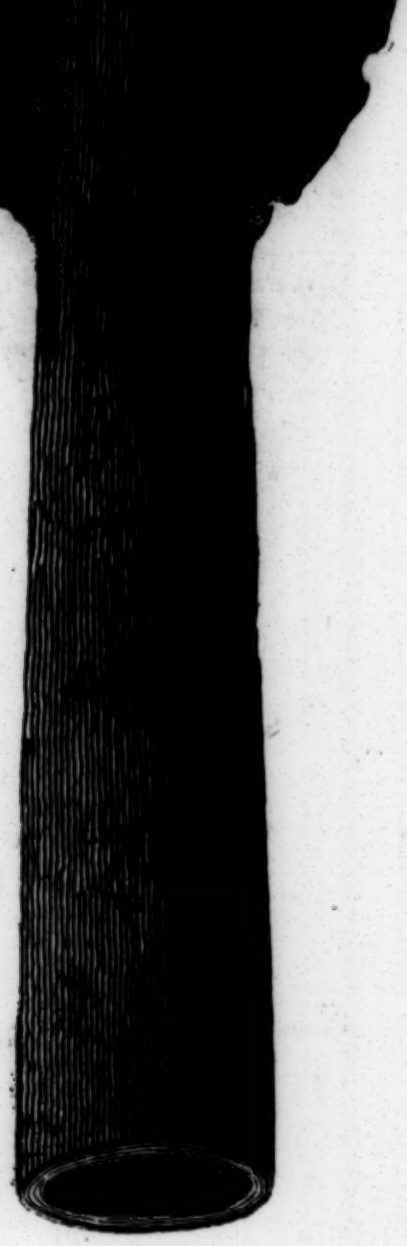


Scale

Fig. 1.



Byron &c.




a

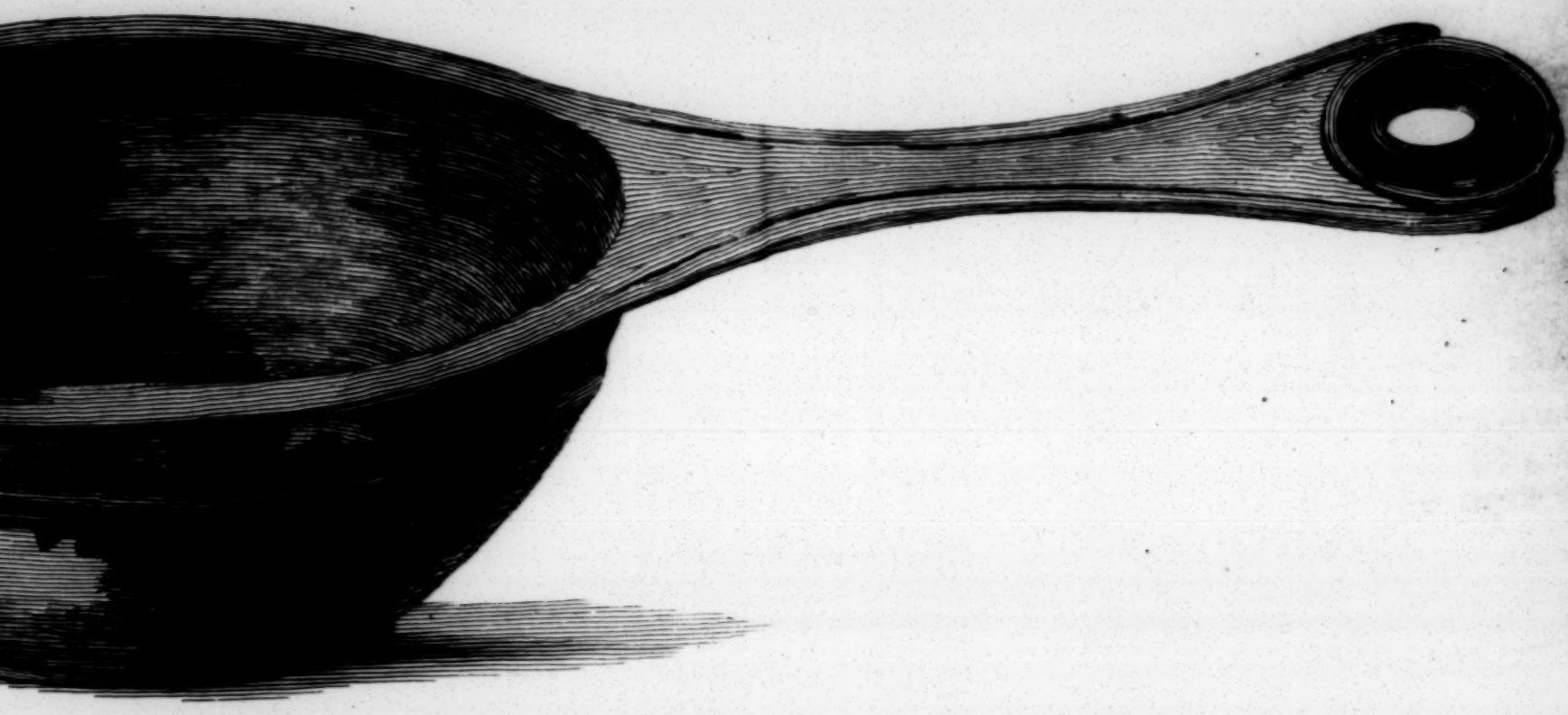

b

a



b







Sculpsit &c.

Fig. 1.

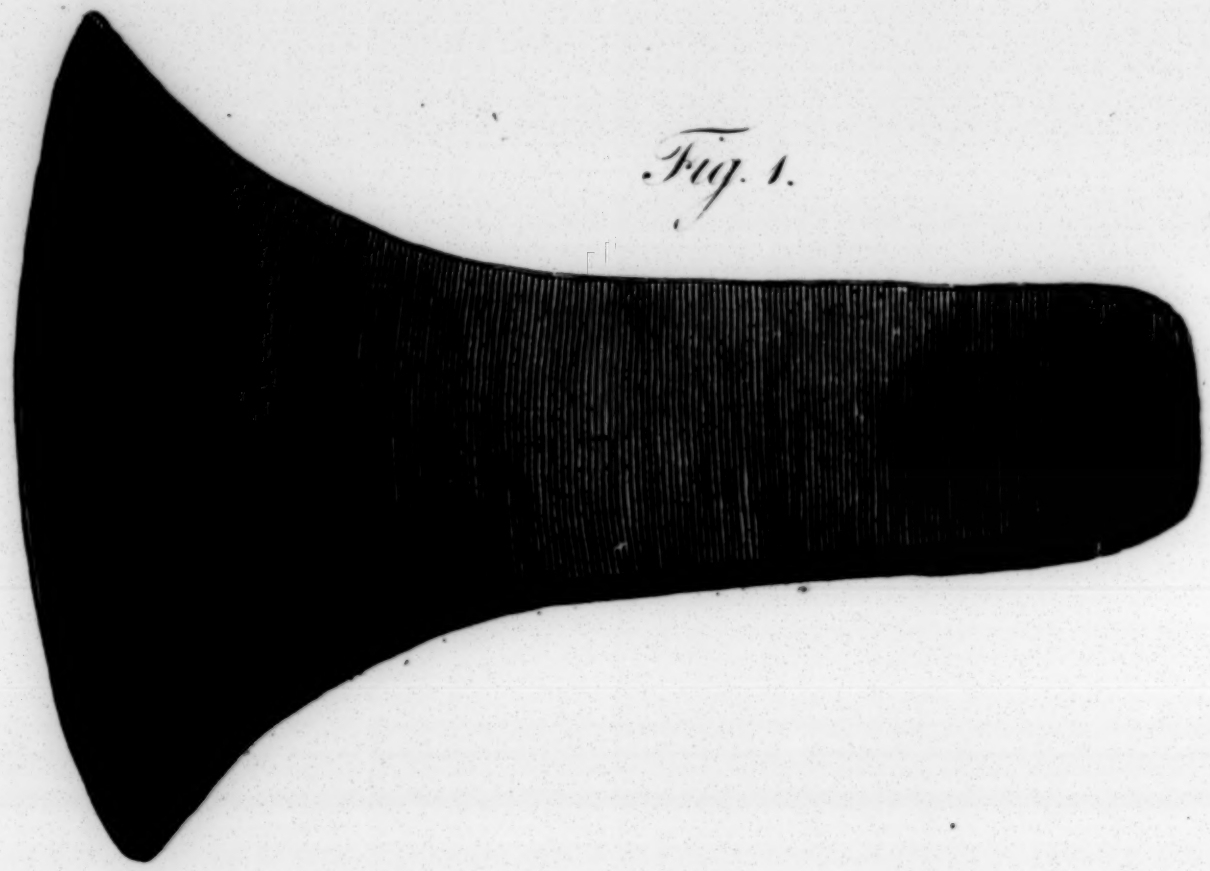


Fig. 2.

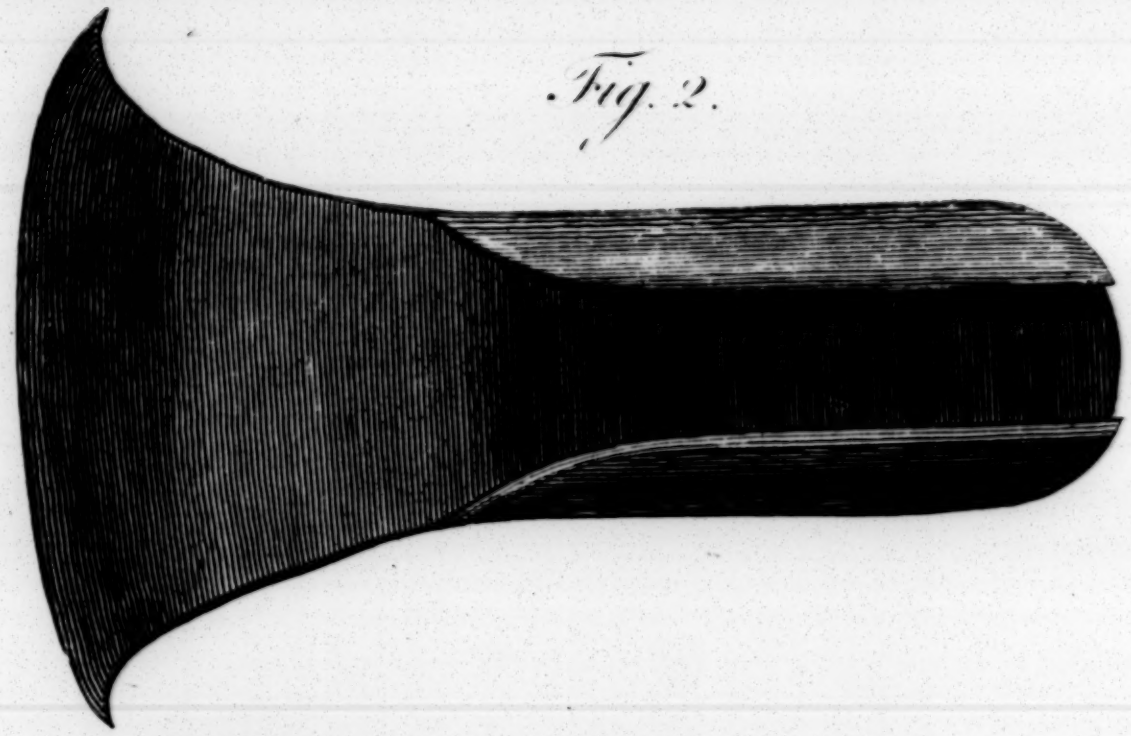
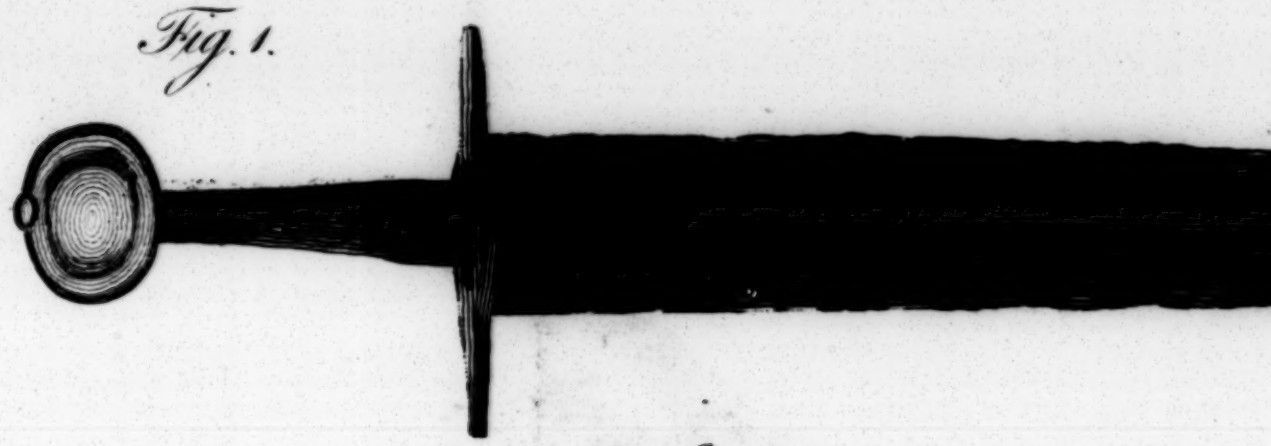


Fig. 3.



Fig. 1.



a

✠BEINVEHVTVS✠

Fig. 3.

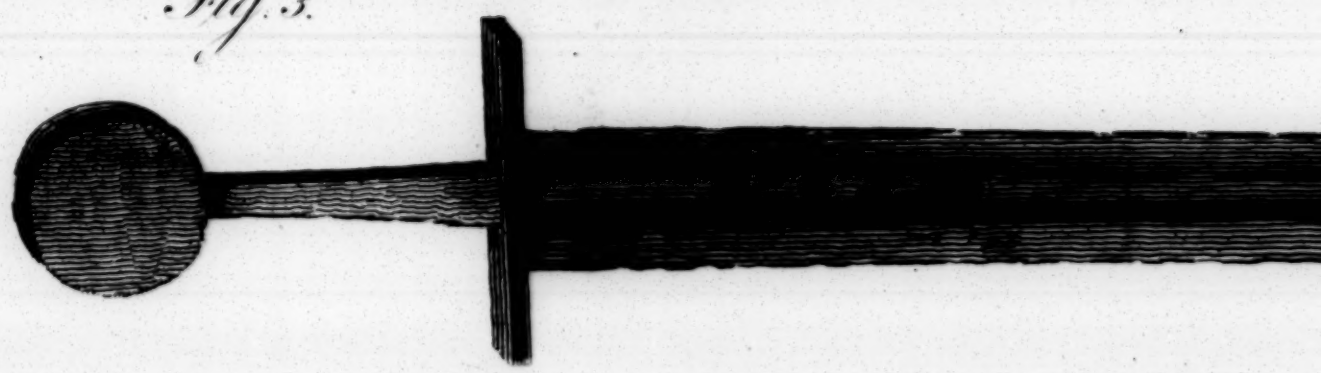
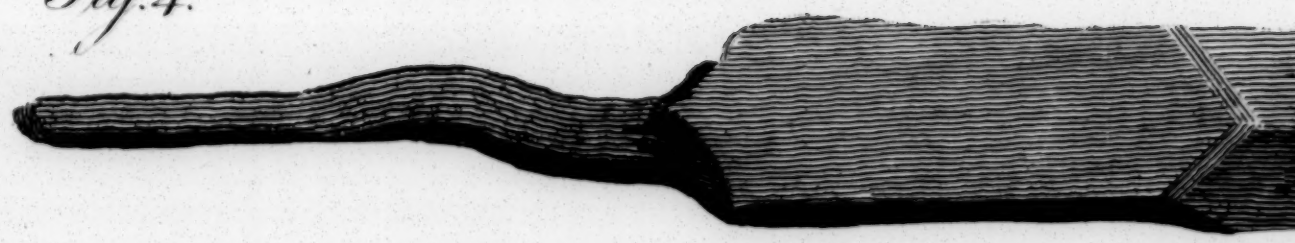


Fig. 4.



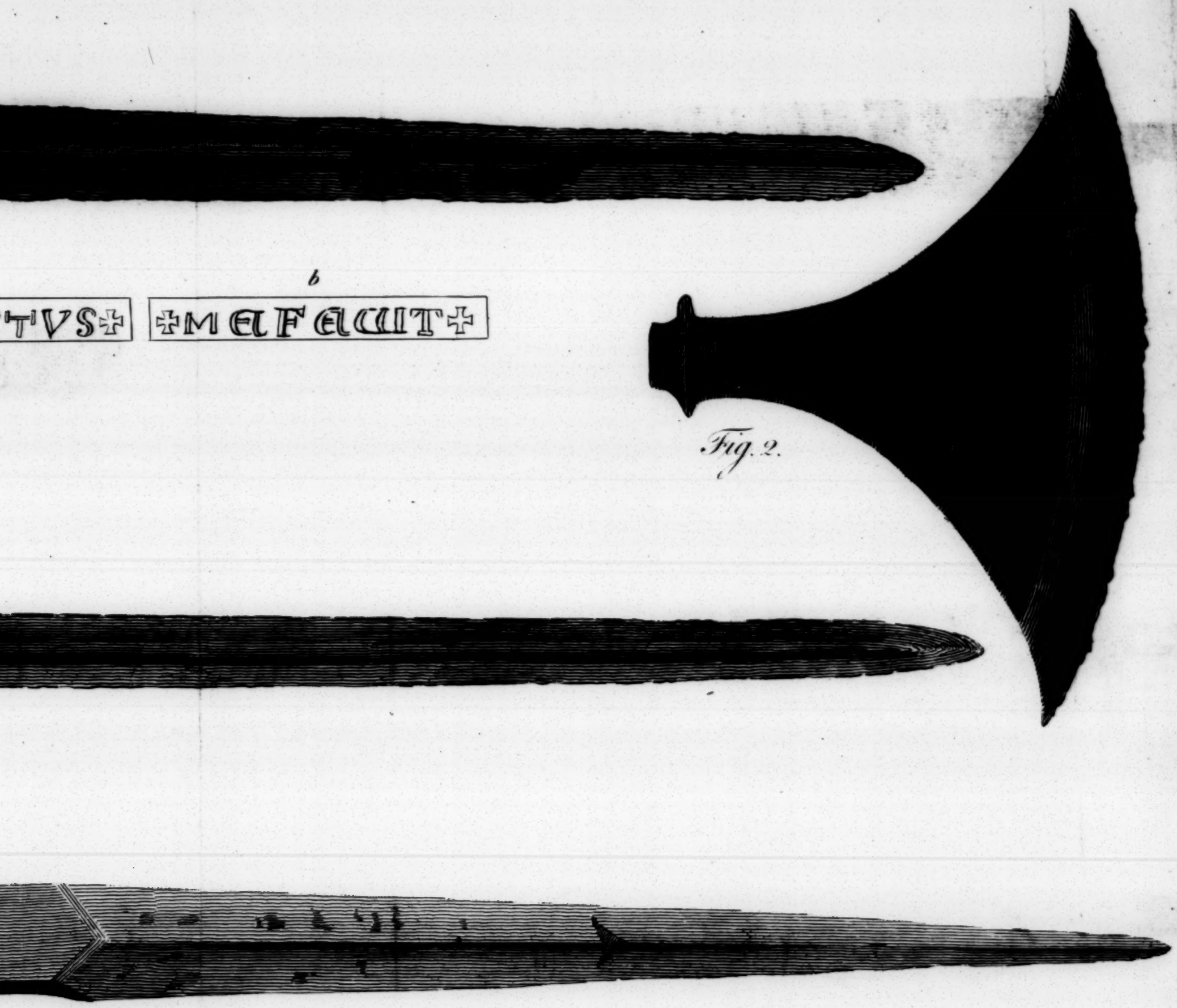


Fig. 2.

Bufire sc.

the union of the oxygen of these substances. Upon this principle the sword, fig. 1. was preserved by its varnish; but the other tools must have owed their preservation to their having been accidentally coated with earthy matter; which perhaps contained principally clay.

9. The destruction of the iron sword by oxygen within the copper scabbard; and the preservation of the part of it not in contact with the copper, is a good example of the action of copper and water united in destroying iron, the copper remaining entire, Sect. I. § 1. IV. This effect of copper upon the iron bolts and nails, in copper-bottomed ships, is a loss of the greatest magnitude.

EXPLANATION OF THE PLATES.

Tab. XI. fig. 1. Two pieces of the Lituus which had been broken.

a. The ornament of chain-work round one end of the Lituus.

Fig. 2. An ancient medal, on which the Lituus is represented.

Fig. 3. Two views of a Danish or Saxon steel Sword in its copper scabbard.

Tab. XII. *a*. The Spear-head, of the same size as the original.

b. A section of its blade, to show what proportion of it is hollow.

Tab. XIII. *a* and *b*. Two views of the Sauce-pan. The diameter of the bowl five inches, and length of the handle five inches.

Tab. XIV. fig. 1, 2, 3, the three Celts, No. 1, 2, 3.

Tab. XV. fig. 1. A steel Sword, the pommel and guard of which were tinned.

a and *b*. The writing in mixed characters on the two sides of the Sword.

Fig. 2. A steel axe.

Fig. 3. Another sword of the same kind, as fig. 1. in Sir JOSEPH BANKS's collection.

Fig. 4. A steel dagger.

XIX. *On the periodical Star α Herculis; with Remarks tending to establish the rotatory Motion of the Stars on their Axes. To which is added a Second Catalogue of the comparative Brightness of the Stars.* By William Herschel, *LL.D. F.R.S.*

Read June 9, 1796.

In my first catalogue of the comparative brightness of the stars, I announced α Herculis as a periodical star. The precision of the characters introduced in that catalogue is such, that the smallest alteration in the lustre of the stars may be discovered, by a proper attention to their expressions: the variation in the light of α Herculis is, however, pretty considerable, and cannot easily be mistaken, when strictly compared to a proper standard. The star most conveniently situated for this purpose is ϰ Ophiuchi; and as I have had no reason, during the time of my observations, to doubt the uniformity of its lustre, I have made use of it in the following comparisons; which seem to be sufficiently decisive, with regard to the periodical variations of the light of α Herculis.

Other stars besides ϰ Ophiuchi have also been consulted; but the unsteadiness of their light would draw me into difficulties, which at present it will be proper to avoid. For this reason I only give the following table, which will be found to contain at least four regular changes of the alternate increase and decay of the apparent lustre of our new periodical star, deduced from a comparison of its brightness with that of ϰ Ophiuchi.

In this short table, the number 64 refers to the stars in the constellation of Hercules, and 27 to those in the constellation of Ophiuchus. The characters that are used to denote their relative lustre, have been explained among the introductory remarks to the first catalogue of the comparative brightness of the stars.

Table of the Variation of Light observed in α (FL. 64) Herculis, compared to ϰ (FL. 27) Ophiuchi.

1795. May	18	64=27	1795. Nov.	20	64 ; 27
	22	64=27		26	64 , 27
July	6	27 ; 64		28	64 ; 27
August	17	27 – 64	Dec.	15	27 ; 64
	18	27 – 64	1796. Jan.	9	64 ; 27
September	16	64 – 27	March	5	64 ; 27
	17	64 – 27		9	64 ; 27
	18	64 , 27		10	64 ; 27
	20	64 ; 27		28	27 , 64
	21	64 ; 27	April	4	27 ; 64
	22	64 , 27		5	27 ; 64
October	2	64 ; 27		9	27 ; 64
	6	64 ; 27	May	2	64 ; 27
	7	64 ; 27		4	64 ; 27
	9	64 . 27		5	64 ; 27
	11	64 ; 27		10	64=' 27
	15	27 , 64		11	64=' 27
	16	27 , 64		14	64=' 27
	17	27 , 64		16	64=' 27
	25	27 ; 64		17	64=' 27
	26	27 ; 64		18	64 – 27
	28	27 , 64		19	64=, 27
	30	27 ; 64		22	64 – 27
November	3	27 , 64		24	64 ; 27
	6	64 . 27		25	64 ; 27
	9	64 ; 27		26	64 ; 27
	10	64 . 27		27	64 , 27

In order now from this table to obtain the time of the period, we shall first take all the successive observations from the 16th of September till the 28th of November. They shew very clearly that the star has completely gone through all its changes. For, admitting a maximum of the light of α Herculis to have been the 16th of September, we find a minimum the 25th of October; and a second maximum about the 28th of November. The period, therefore, is of somewhat more than two months duration.

But as changeable stars are subject to temporary inequalities, which will render a determination of the length of a period, from a single series of changes, liable to considerable errors, we shall now take the assistance of the most distant observations. By an inspection of the table, we find again the first maximum to have been about the 16th of September, 1795; and the fourth the 14th of May, 1796. This being an interval of 241 days, in which four successive changes have been gone through, we obtain about 60 days and a quarter for the duration of the period.

In confirmation of this computation, the table shews that our periodical star was very faint in August, 1795; bright about the middle of September; faint towards the end of October; bright the latter part of November; faint in December; bright in January, 1796; not observed in February; bright in March; faint in April; and lastly, bright again in May. This is just what should have happened according to the above determination, which, as we have seen, gives a period of eight weeks, four days and a quarter. Greater accuracy can only be obtained by future observations.

On the Rotatory Motion of the Stars on their Axes.

I shall now add a few remarks on the subject of the rotation of the fixed stars on their axes. This motion has been lately mentioned in a paper, where I could not have an opportunity to enter into the reasons why it ought to be admitted.* The discovery of the period of α Herculis furnishes me with an opportunity to say a few words upon the subject, as every addition to the list of periodical stars increases our knowledge of the construction of the celestial bodies. Not so much because now one star more is known to be subject to periodical changes in its lustre; for this would indeed be of no great consequence. But we ought not to be satisfied with merely inrolling this circumstance among the list of facts we are acquainted with. The rotatory motion of stars upon their axes is a capital feature in their resemblance to the sun. It appears to me now, that we cannot refuse to admit such a motion, and that indeed it may be as evidently proved as the diurnal motion of the earth.

Dark spots, or large portions of the surface, less luminous than the rest, turned alternately in certain directions, either towards or from us, will account for all the phænomena of periodical changes in the lustre of the stars, so satisfactorily, that we certainly need not look out for any other cause. Let us, however, take a review of any objections that might be made.

The periods in the change of the lustre of Algol, β Lyræ, δ Cephei, and η Antinoi, are short; being only 3, 5, 6, and 7 days respectively: those of o Ceti, the changeable star in

* Phil. Trans. for the year 1795, Part I. page 68.

Hydra, and that in the neck of the Swan are long, amounting to 331, 394, and 497 days. Will not a doubt arise whether the same cause can be admitted to explain indiscriminately phænomena that are so different in their duration?

To this it may be answered, that the whole force of the objection is founded upon our very limited acquaintance with the state of the heavens. Hitherto we have only had seven stars whose periodical changes have been determined. No wonder then that proper connections between their different periods were wanting. But let us now place α Herculis among the list, which is not less than 60 days in performing one return of its changes. Here we find immediately, that the step from the rotation of α Herculis to that of o Ceti, is far less considerable than that from the period of Algol to the rotation of α Herculis; and thus a link in the chain is now supplied, which removes the objection that arose from the vacancy.

There is, however, another instance of a slow rotatory motion; and it is doubly instructive upon this occasion. In one of my former papers it has been shewn, that the 5th satellite of Saturn revolves on its axis in 79 days; this not only shews that very slow rotatory motions take place among the celestial bodies; but from the arguments that were brought to prove its rotation, which I believe no astronomer will oppose, we are led to apply the same reasoning to similar appearances among the fixed stars. A variation of light, owing to the alternate exposition of a more or less bright hemisphere of this periodical satellite, plainly indicates that the similar phænomenon of a changeable star, arises from the various lustre of the different parts of its surface, successively turned to us by its rotatory motion.

The rotations of the sun and moon, and of several of the planets, become visible in a telescope by means of the spots on their surfaces; the remote situation and smallness of the 5th satellite of Saturn, leave us without this assistance; but what we can no longer perceive, with our best optical instruments, we now supply by rational arguments. The change in the light of the satellite proves the rotation; and the rotation once admitted, proves the existence of spots, or less luminous regions on its surface, which at setting off were only hypothetical. In the same manner a still more extended similarity between the sun and the stars offers itself, by the spots that now must also be admitted to take place on their surfaces, as well as on that of the sun.

To return to the difficulty which has been started, it may be further urged, that there are some reasons to surmise that the 34 Cygni is a periodical star of 18 years return;* and that other stars seem very slowly to diminish their lustre, and may probably recover it hereafter.

In answer to this, I remark that it will not be necessary to remove objections to the rotatory motion of the stars, inferred from their very slowly changeable lustre, till they come properly supported by well ascertained facts. Many causes in the physical construction of the stars may occasion an accidental and gradual increase or decay of brightness, not subject to any regularity in its duration. But when settled periods can be ascertained, notwithstanding they should be of the most extended duration, it will not be difficult to find other causes to explain them, without giving up the rotatory motion. When the biography of the stars, if I may be allowed the expression,

* Phil. Trans. for the year 1786, Part I. page 201.

is arrived to such perfection as to present us with a complete relation of all the incidents that have happened to the most eminent of them, we may then possibly not only be still more assured of their rotatory motion, but also perceive that they have other movements, such as nutations or changes in the inclination of their axes; which, added to bodies much flattened by quick rotatory motions, or surrounded by rings like Saturn, will easily account for many new phænomena that may then offer themselves to our extended views.

Memorandum relating to the following Catalogue.

It was my intention to have continued to remark all the deviations of brightness, here assigned to the stars, from the magnitudes which are given by FLAMSTEED, either in his observations, or in the British catalogue; but I now find this author so little consistent, that it appears to be of no use to refer to his determinations. In the constellation of Aries are no less than 31 stars, in which the magnitudes of his catalogue differ from those we find in his observations; and these are generally the stars of the greatest brightness. The difference is also very considerable: thus, 41 Arietis is observed 6m, the catalogue gives 3m; 50 is observed 7m, the catalogue gives 5m; 35 is 5m, catalogue 4m; 33 is 6m, catalogue 5m; 38 is 6m, catalogue 7m.

The notes to these and the remaining constellations, therefore, will now be confined chiefly to my own observations, and to the correction of errors that have fallen under my notice.

II. *Catalogue of the comparative Brightness of the Stars.*

Lustre of the stars in Aries.			
1		7.6	Does not exist.
2		7.6	2, 8 2; 8
3		6	4, 3
4		7.6	8 – 4, 3
5	γ	4	41.5 5 – 3 Aurigæ 92 Ceti, 5 41 – 5 5; 37 Cassiop 99 Pisc 5 –, 110 Piscium
6	β	3	6 – 4 Trianguli 88 Pegasi; 6
7		tel.	14 – 7
8	ι	6	17.8.22 2; 8 – 4
9	λ	5	9, 12 9; 12 9, 14
10		6.7	14, 10 – 21
11		6	20, 11.16
12	κ	6.5	9, 12 9; 12 12 – 17
13	α	2	57 Andro; 13 13, 23 Aurigæ 43 Andromedæ; 13
14		6	14, 10 9, 14 – 7
15		6	22.15 –, 18
16		8	11.16
17	η	6	17.8 12 – 17, 22
18		7	15 –, 18.23
19		6.7	20, 19
20		6	21.20, 11 20, 19
21		7	10 – 21.20
22	θ^1	6.5	8.22 17, 22.15
23	θ^2	7	18.23
24	ξ	6	31, 24 65 Ceti, 24, 64 Ceti
25		7	64 Ceti – 25
26		6.7	29, 26, 27

Lustre of the stars in Aries.			
27		6.7	26, 27
28		6	Does not exist, or is lost
29		6.7	29, 26
30		7	35, 30 – 33 16 Trianguli, 30
31		5.6	38 – 31, 24
32	ν	6	48 =' 32 ; 34
33		5	30 – 33 35, 33 33 –, 16 Trianguli
34	μ	6	32 ; 34
35		4	39, 35, 33 35, 30 35 · 57
36		7	53; 36 · 50
37	o	6	43 · 37
38		7	38 – 31
39		4	39, 35
40		6	46 · 40, 45
41		3	41 · 5 41 – 5
42	π	6	42 ; 46 42, 43
43	σ	6	42, 43 · 37
44	ρ^1	6	54, 44
45	ρ^2	6.7	40, 45 · 53
46	ρ^3	6.7	42 ; 46 · 40
47		6.7	63 · 47, 65
48	ε	5	58 · 48 · 63 48, 57 48 =' 32
49		7	52 ; 49 · 51
50		7	36 · 50 · 54
51		7	49 · 51
52		6	52 ; 49 52, 56
53		7	45, 53 ; 36
54		6.7	50 · 54, 44
55		7	56 · 55
56		6.7	52, 56 · 55
57	δ	4	35 · 57 · 58 48, 57, 58

Lustre of the stars in Aries.			
58	ζ	5	57 . 58 . 48 57, 58, 61
59		7	62 – 59, 60
60		7	59, 60 . 64
61	τ^1	7	58, 61, 63
62		6	62 – 59
63	τ^2	6	48 . 63 61, 63, 65 63 . 47
64		6	60 . 64 – 66
65		7	63, 65 47, 65
66		7	64 – 66 7 Tauri . 66
Lustre of the stars in Canis major.			
1	ζ	3	1 . 24
2	β	2	21, 2, 25
3	λ	4	3 . 13 3; 8 Columbæ
4	ξ^1	5	4, 5
5	ξ^2	5	4, 5
6	ν^1	5	8 – 6
7	ν^2	5	7, 8 7, 23
8	ν^3	5	7, 8 – 6
9	α	1	9 – – – all 1 m
10	$\varkappa^1$	6	13 – 10
11		5	18 . 11
12	ρ	6	17 . 12
13	$\varkappa^2$	5	3 . 13 – 10
14	θ	5	14, 20
15	π^1	6	19 . 15 – 17
16	o^1	5	22, 16, 28
17	π^2	6	15 – 17 . 12
18	μ	4	20, 18 . 11
19	π^3	6	19 . 15
20	ι	4	23, 20 14, 20, 18

Lustre of the stars in Canis major.			
21	ε	3.2	21, 2 21−2
22		4	22, 16
23	γ	3	7, 23, 20
24	o^2	5.4	1.24
25	δ	2.3	2, 25, 31
26		7	27, 26
27		7	28−27, 26
28		5	16, 28−27 28−30
29		5	30.29
30		5	28−30.29
31	η	3.2	25, 31 15 Navis, 31
Lustre of the stars in Canis minor.			
1		7.6	6, 1 7, 1 11, 1
2	ε	6	4, 2, 5
3	β	3	3−−4
4	γ	6	3−−4, 2 4.6
5	η	6	2, 5
6	o	6	4.6, 1 6, 11
7	δ^1	6	7, 1 7, 9
8	δ^2	6	9.8
9	δ^3	6	7, 9.8
10	α	1.2	19 Orionis −, 10 58 Orionis . 10 19 Orionis −, 10
11	π	6	6, 11, 1
12		5.6	Does not exist.
13	ζ	5	13 Navis, 13−14 13 Navis − 15
14		6	13−14

Lustre of the stars in Cassiopea.			
1	*e*	6	3, 1 -- 2
2		7	1 -- 2
3		6	7 . 3, 1 7 . 3 . 8
4	*d*	5	5, 4 - 6
5	τ	5	8 - 5, 4 5 - 12 5, 14
6		6	4 - 6 . 9
7	ρ	6	7 . 3
8	σ	6	3 . 8 - 5
9		6	6 . 9 . 10
10		6	9 . 10
11	β	3 . 2	27, 11 27 . 11
12		6	5 - 12 - 13 14, 12
13		6	12 - 13 . 16
14	λ	5	17 -- 14 33, 14 5, 14, 12
15	$\varkappa$	4	15, 33
16		6	13 . 16
17	ζ	4	24, 17 -- 14 24 - 17
18	α	3	18 ; 27 18 . 27
19	ξ	6	19 - 25
20	π	6	22, 20
21		6	21 ; 23
22	*o*	6	22, 20
23		6	21 ; 23
24	η	4	24, 17 24 -- 33 24 - 17
25	ν	5	19 - 25
26	υ^1	7	28 ; 26
27	γ	3	18 ; 27, 11 18 . 27 . 11
28	υ^2	6	28 ; 26
29		6	Does not exist.
30	μ	5	33 = $\overline{\tau}$ 30
31		6	36 - 31, 43 46, 31

Lustre of the stars in Cassiopea.			
32		6	32 – 35
33	θ	4	24 – – 33 , 14 33 = ∓ 30 15 , 33
34		6	44 – 34
35		7	32 – 35
36	Ψ	5.6	36 – 31 36 , 46
37	δ	3	5 Arietis ; 37 – – 45
38		6	42 – 38 40 , 38
39	χ	6	39 , 44
40	*b*	6	42 , 40 , 38
41		6	Does not exist.
42	*g*	6	48 – 42 – 38 42 , 40
43	*c*	6	31 , 43
44		6	39 , 44 – 34
45	ε	3	37 – – 45
46	*d*	6	36 , 46 , 31
47		5	47 , 49
48	*e*	5	50 – 48 – – 54 48 – 42
49		6	47 , 49
50	*f*	4.5	50 – 48
51		6	Does not exist, or is lost.
52		7	53 ∓ 52 53 , 52 – 55
53		7	53 ∓ 52 53 , 52
54		6	48 – – 54
55		6	52 – 55
Lustre of the stars in Cetus.			
1	.	6	6 , 1 9 , 1
2	*g*	4.5	2 , 7
3		6	17 , 3 , 19
4		6	4.5
5		6	4.5

Lustre of the stars in Cetus.			
6	*f*	5	7-6, 1
7	*b*	5	2, 7-6
8	*i*	3	8. 31
9		6	9, 1
10		6	12-10, 11 12, 10 44 Piscium-10
11		6	10, 11
12		6	13, 12-10 13, 12. 15 13-12, 10
13		6	13, 12 20, 13, 12 13-12
14		6	Does not exist.
15		6	12. 15
16	β	3	8 Pegasi ; 16 ; 44 Pegasi
17	φ^1	5	17, 19 17, 3
18		6	23, 18
19	φ^2	5	17, 19-22 3, 19
20		6	20, 13 20-26
21		6	27. 21
22	φ^3	5	19-22. 23
23	φ^4	5	22. 23, 18
24		6	Does not exist.
25		6	25. 37
26		6	20-26, 33
27		6	28, 27. 30 27. 21
28		6	37, 28, 27
29		6	35, 29
30		6	27. 30, 32
31	η	3	8. 31. 45
32		6	30, 32. 41 32. 44
33		6	26, 33, 35
34		6	34, 38
35		6	33, 35, 29
36		6	36. 41

Lustre of the stars in Cetus.			
37		5	25 . 37 , 28
38		6	34 , 38 . 39
39		6	38 . 39 , 40
40		6	39 , 40 . 42
41		6	32 . 41 36 . 41
42		6	40 . 42 . 43
43		6	42 . 43
44		6	32 . 44
45	θ	3	31 . 45 45 , 52
46		5	50 , 46 48 . 46 53 – 46
47		6	47 , 49
48		6	48 . 46
49		6	47 , 49 . 50
50		6	49 . 50 , 46
51		6	110 Piscium – 51 , 98 Piscium
52	τ	3 . 4	45 , 52 . 55
53	χ	5	55 – – 53 – 46
54		6	64 , 54
55	ζ	3	52 . 55 – – 53
56	υ^1	4	59 – 56 , 57
57		5	56 , 57 57 . 74
58		6	58 , 66
59	υ^2	4 . 5	59 – 56
60		6	60 – 61
61		7	60 – 61 , 63
62		6 . 7	63 – 62 71 . 62
63		6	66 , 63 – 62 61 , 63
64		6	65 – – 64 , 54 65 – , 64 24 Arietis , 64 – 25 Arietis
65	ξ^1	6	65 – – 64 73 . 65 – , 64 65 , 91 65 , 24 Arietis

Lustre of the stars in Cetus.			
66		6	66, 63 58, 66, 63
67		6	80, 67
68	o	3.2	68 < 10 m 68 = 13 Arietis 68 > 13 Arietis 87 Tauri > 68
69		6	69 . 70
70		6	69 . 70, 75
71		6	71 . 62 81, 71 - 79
72	ρ	4	83 . 72
73	ξ^2	5.4	87, 73 . 65 73 - 91
74		6	57 . 74
75		5.6	70, 75, 84
76	σ	4	89 - 76 . 83
77		6	77 . 80
78	ν	4.5	96, 78, 97 88, 78 - 85
79		6	71 - 79
80		6	77 . 80, 67
81		6	81, 71
82	δ	3	85 - 82
83	ε	3	90 . 83 76 . 83 . 72
84		6	75, 84
85		6	78 - 85 88 -, 85
86	γ	3	86 - 82
87	μ	4	87, 73
88		6	88, 78 88 -, 85
89	π	4.3	89 . 90 89 - 76
90		8	89 . 90 . 83
91	λ	4	73 - 91 65 ; 91
92	α	2	92, 4 Arietis
93		6	96 - 93, 97
94		6	96, 94 - 95
95		6	94 - 95

Lustre of the stars in Cetus.

96	κ^1	5	96, 78	96, 94	96-93
97	κ^2	4	78, 97	93, 97	

Lustre of the stars in Corvus.

1	α	4	7-1-8	
2	ε	4	9, 2. 7	
3		6	5-3-, 6	
4	γ	3	4, 9	
5	ζ	5	8-5-3	
6		6	3-, 6	
7	δ	3	9, 7-1	2. 7-1
8	η	5	1-8-5	
9	β	3	4, 9, 7	4, 9, 2

Lustre of the stars in Eridanus.

1	τ^1	4	11, 1-2	
2	τ^2	4	1-2, 4	
3	η	3	18. 3	
4		6	2, 4, 6	15, 4
5		6	5-, 7	
6		6	4, 6	
7		6	5-, 7	
8	ρ^1	6	10. 8	
9	ρ^2	5	13-9, 10	13-9-14
10	ρ^3	4	9, 10. 8	
11		3. 4	16, 11, 1	16, 11. 27
12		3	19, 12	
13	ζ	3	13-9	
14		6	9-14	
15		6	15, 4	15-20

Lustre of the stars in Eridanus.			
16		4	16, 11
17		4.5	17–22
18	ε	3.4	23, 18–26 23, 18. 3
19		4	27, 19, 12
20		5.6	15–20
21		6	22; 21
22		5.6	17–22; 21
23	δ	3.4	34–, 23, 18 53, 23
24		5	32, 24; 25 24–25
25		6	24; 25 24–25
26	π	4	18–26, 32
27		4	11. 27, 19 27–33
28		6.5	36, 28
29		6.7	30, 29
30		5.6	32–30, 29
31		5.6	Does not exist
32		4.5	26, 32–30 26, 32–, 35 32, 24
33		4.5	27–33. 36
34	γ	2	67, 34–, 23
35		5	32–, 35
36		4	33. 36, 28
37		6	40. 37
38	ο	3.4	38, 40
39	A	5	40. 39, 42
40	*d*	5	38, 40–37 40. 39
41		4.3	41, 43
42	ξ	4.3	39, 42
43		5	41, 43
44		5.6	49. 44
45		5.6	45. 49
46		5	46. 47

Lustre of the stars in Eridanus.			
47		4	46 . 47 , 56
48	ν	4	48 . 57 48 –, 51
49		5.6	45 . 49 . 44
50	υ^1	4	50 . 52 52 , 50
51	c	4	57 –, 51 48 –, 51
52	υ^2	3	50 . 52 52 , 50
53		3 . 4	53 , 23
54		3 . 4	54 – 60
55		6	56 , 55
56		6	47 , 56 , 55 63 – 56
57	μ	4	48 . 57 –, 51
58		5.6	60 , 58 . 59
59		6	58 . 59
60		6	54 – 60 , 58
61	ϖ	5	69 , 61 , 65
62	b	6	65 –, 62
63		6	64 – 63 – 56
64		6	64 – 63
65	Ψ	5	61 , 65 –, 62
66		6	68 . 66
67	h	3	67 , 34 67 – – 68
68		6	67 – – 68 . 66
69	λ	4	69 , 61

Lustre of the stars in Gemini.			
1	H	5	18 – 1 – 139 Tauri
2		8	5 – 2 . 4
3		8	3 , 5
4		7	2 . 4 . 6
5		7	3 , 5 – 2
6		7	4 . 6

Lustre of the stars in Gemini.			
7	η	4.5	13, 7 – 31 13 : 7 –, 31
8		7.8	8 . 9
9		7	8 . 9 , 10
10		8	9 , 10 . 11
11		8	10 . 11 . 12
12		8	11 . 12
13	μ	3	27 ; 13 , 7 13 , 27 13 ; 7
14		7.8	16 . 14 . 15
15		7	14 . 15 , 17
16		7	16 . 14
17		8	15 , 17
18	ν	4	69 . 18 – 1
19		7	20 , 19 , 23
20		7.8	20 , 23 20 , 19 20 – 23
21		6.7	Does not exist.
22		7	23 . 22
23		5	20 , 23 19 , 23 , 22 20 – 23
24	γ	2 . 3	50 Orionis , 24 , 37 Aurigæ 66 –, 24
25		7	54 Aurigæ – 25
26		5	26 . 61
27	ε	3	27 ; 13 13 , 27
28		6	53 Aurigæ . 28 , 54 Aurigæ 36 , 28
29		6.7	Does not exist.
30	ξ^1	6	31 - 30 . 38
31	ξ^2	5.4	7 – 31 – 30 7 –, 31
32		6	35 , 32
33		6 . 7	35 · 33
34	θ	4	77 , 34 – 43
35		6	38 , 35 , 32 35 · 33
36	d	6	36 , 28 36 , 37
37		6	36 , 37 , 39

Lustre of the stars in Gemini.			
38	*e*	6	30 . 38 − 35
39		6 . 7	37 , 39 . 40
40		6	39 . 40
41		6	45 − 41 45 , 41 , 50
42	ω^1	6	42 −, 44
43	ζ	3 . 4	34 − 43
44	ω^2	6 . 7	42 −, 44
45	*o*	6	51 − 45 − 41 68 , 45 , 41
46	τ	5	60 − 46 69 , 46 62 − 46
47		6	48 , 47 , 52
48	*m*	6	57 − 48 , 47
49		7	52 − 49 58 , 49
50		6	41 . 50
51		6 . 5	51 − 45 51 , 68
52	*n*	7 . 6	47 . 52 − 49 52 , 58
53		6 . 7	59 . 53
54	λ	5	54 ; 55
55	δ	3	55 . 77 54 ; 55
56	*q*	6 . 7	56 ; 63
57	A	5 . 6	57 − 48
58		7 . 8	52 , 58 , 49
59		6	64 − 59 59 . 53
60	ι	4 . 5	60 − 65 60 − 46 60 , 62
61	*r*	6	26 . 61 63 , 61
62	*s*	5	60 , 62 . 46
63	*p*	6	56 ; 63 , 61
64	b^1	6	65 . 64 − 59
65	b^2	6	60 − 65 . 64
66	α	1	78 −, 66 −, 24 66 . 34 Aurigæ
67		7 . 8	68 −, 67
68	*k*	6	68 −, 67 51 , 68 , 45 68 , 81

Lustre of the stars in Gemini.			
69	υ	5	69 . 18 69 , 46
70		5	80 – 70
71	o	5	71 . 80
72		6	Does not exist.
73		6 . 7	Does not exist.
74	*f*	6	81 , 74 – 85
75	σ	5	75 , 83
76	*c*	6	83 – 76
77	ϰ	4 . 5	55 . 77 , 34
78	β	2	78 –, 66 78 , 67 Virginis 87 Tauri – – 78
79		7	82 . 79 , 84
80	π	5	71 . 80 – 70
81	*g*	6	68 , 81 , 74
82		6	85 – 82 , 84 82 . 79
83	φ	5	83 – 76 75 , 83
84		5	82 , 84 79 , 84
85	*l*	6	74 – 85 – 82
Lustre of the stars in Leo.			
1	ϰ	4	4 ; 1 1 –, 22
2	ω	5	6 – 2 ; 3
3		6	2 ; 3
4	λ	4	4 ; 1
5	ζ	4	10 , 5 – 6
6	*b*	6	5 – 6 – 2
7		6	16 – 7 , 11 8 , 7
8		6	16 , 8 , 7
9		6	13 , 9 – 12
10		5	10 , 5
11		6	7 , 11
12		7 . 8	9 – 12

Lustre of the stars in Leo.			
13		6	13, 9
14	o	3·4	14, 47 14, 30
15	*f*	6	22 . 15
16	ψ	6	16 – 7 16, 8
17	ε	3	68 – – 17 – 70 17 – 36 17 – 30 68 –, 17 –, 36 17 –, 70
18		6	18, 23
19		7	23 . 19, 21
20		6	22 –, 20
21		7	19, 21
22	*g*	6	22 . 15 1 –, 22 –, 20
23		6	18, 23 . 19
24	μ	3·4	47, 24
25		6.7	Does not exist.
26		7	Does not exist.
27	ν	4·5	29 – 27
28		7	Does not exist.
29	π	4	29 – 27 31 – 29
30	η	3·4	70 . 30 17 – 30 36 . 30 14 . 30
31	A	5	31 – 29 31 – 40
32	α	1	78 Geminorum –, 32, 66 Geminorum
33		6.7	Lost or 34 – – 33
34		7	42 – 34
35		6	39 ; 35
36	ζ	3	17 – 36 . 70 70 . 36 . 30 36 – 46 Leonis minoris 17 –, 36
37		6	37 – 42
38		6	Does not exist.
39		6	39 ; 35
40		6	31 – 40
41	γ	2	41, 94 41 – 94 41 –, 94

Lustre of the stars in Leo.			
42		6	37 – 42 – 34
43		6	44 , 43
44		5.6	44 , 45 49 , 44 , 43
45		6	44 . 45 46 , 45
46	*i*	6	46 – 47 Leonis minoris 46 , 45 46 – 50 46 . 51
47	ρ	4	14 , 47 , 24
48		6	48 , 49
49		6	48 , 49 , 44
50		7.6	46 – 50
51	*m*	6	46 . 50
52	K	6	41 Leonis minoris – 52 53 , 52
53	*l*	6	53 , 52
54		4.5	60 . 54 –, 72
55		6.5	55 – 57 55 , 62
56		6.7	59 –, 56
57		6	55 – 57 62 , 57
58	*d*	6.5	63 – 58 ; 59
59	*c*	5	58 ; 59 59 –, 56
60	*b*	5	60 . 54
61		5	61 , 69
62	*g*	6	55 , 62 , 57
63	χ	4.5	63 – 58
64		6	67 – 64
65		6	79 , 65 , 76
66		6	69 – 66
67		6	72 –, 67 – 64
68	δ	2.3	94 – 68 – – 17 68 –, 17
69		5.6	61 , 69 – 66
70	θ	3	17 – 70 36 . 70 . 30 17 –, 70 . 36
71		6	Does not exist.

Lustre of the stars in Leo.			
72		5	54–, 72–, 67
73	*n*	6	73 ; 81
74	φ	4	91 . 74–, 87
75		6	84, 75, 79
76		7	79–76 65, 76
77	σ	4.5	77, 78
78	ι	4	77, 78–91
79		5.6	75, 79–76 79, 65
80		6	89–80–83
81		6	73 ; 81 86–81, 90
82		7.8	83, 82
83		8	80–83, 82
84	τ	4	84, 75
85		6	85, 88 90, 85
86		6	86–81
87	*e*	4.5	74–, 87
88		6	85, 88
89		6	89–80
90		6	81, 90, 85
91	υ	4	78–91 . 74
92		6	95, 92
93		4	93–, 95
94	β	1.2	41, 94 41–94 41–, 94–68
95	ο	6	93–, 95, 92

Notes to Aries.

1 There is no observation of this star in FLAMSTEED's works. Perhaps one of the observations of 104 Piscium was placed 8 degrees too much north, which would produce the first Arietis.

2 There are two stars, the largest of which I take to be the 2d Arietis, and have estimated its brightess.

28 There is an observation in the second volume of the *Historia Cœlestis*, page 181; and in all appearance it is a good one. But as we find no where the 26th and 28th observed together, it is probable that only one of them existed in FLAMSTEED's time. The observation, Dec. 10, 1692, being corrected — 1′ of time in RA, will agree with the four observations on page 81, 181, 273, and 283.

Catalogue and Atlas.

1 and 28 should be out.

Atlas.

A small star under 41 towards 16 Trianguli should be out.

Notes to Canis major.

" Order of magnitude Nov. 17, 1784. ε 2m β 2 . 3m δ 3 . 2 m η 3m o 4m."

2 " Is 3 m, January 9, 1796."

23 " Is hardly 5 m, January 9, 1796."

Notes to Canis minor.

3 " Is 4m, January 9, 1796."

10 The comparative brightness at first sight is 10 – 19 Orionis; but the light of Rigel being more brilliant than that of Procyon, a continuance of its impression, when we look a good while, occasions an increase; and it becomes 19 Orionis ⁊ 10.

12 FLAMSTEED never observed this star.

Catalogue and Atlas.

12 should be out.

Notes to Cassiopea.

" Order of magnitude May 12, 1783. $\beta\,\alpha\,\gamma$ 2″m $\delta\,\epsilon$."

8 There are two stars, nearly equal.

29 There is no observation of FLAMSTEED of this star.

41 There is no observation of this star by FLAMSTEED.

51 FLAMSTEED observed this star Dec. 16, 1691. The observation, however, is marked defective in zenith-distance. There is a star, about 9 or 10 m, near the place where 51 should be.

55 That which I have estimated is the largest of several small stars about the place of 55.

Catalogue and Atlas.

29, 41, and 51 should be out.

52 and 53 are not laid down in Atlas as they are in the heavens; and on examining FLAMSTEED's observation on page 208, it appears that the catalogue is erroneous; for by that observation 52 is the most north, and 53 the most south, contrary to the catalogue.

Notes to Cetus.

14 There is no observation of this star by FLAMSTEED.

24 There is no observation of this star by FLAMSTEED.

51 Is 106 Piscium.

68 A periodical star of a great range in its lustre, as appears from the expressions I have given of its comparative brightness. Its period is 331 days, 10 hours, 19 minutes. See Phil. Trans. 1792, Part I. page 25. Different authors, however, vary a little in their determinations of the length of this period; for which we may account by admitting the star to be subject to considerable alterations in the emission of light, from some parts of its surface, which being more copious sometimes in one place, and sometimes in another at some small distance, will give a different result to the observations of the time of its maximum; while, notwithstanding, the general period of its changes will not be considerably affected by it. We have a similar instance in the rotation of Jupiter, which seems to vary on account of the little stability of its spots. See Phil. Trans. 1781, Part I. page 123 to 126.

90 Is 1 Eridani.

Catalogue and Atlas.

14 and 24 should be out.

54 " requires + 1′ 8″ of time in RA. Sept. 4, 1786."

Atlas.

18 " Is not laid down very accurately in Atlas, Sept. 22, 1795."

Notes to Corvus.

" Order of magnitude May 12, 1783. $\gamma\,\delta\,\beta\,\alpha$."

The order, by the present catalogue, does not agree with that in 1783. It is no longer $\delta\beta$, but $\beta\,\delta$; but the constellation is so low that precision is difficult.

Notes to Eridanus.

1 Is 90 Ceti.

31 FLAMSTEED has no observation of this star.

Catalogue and Atlas.

31 must be out.

40 " requires +6′ in PD, January 31, 1786."

Notes to Gemini.

21 There is an observation by FLAMSTEED on page 294, but with a small correction, minus, in time it will agree with 20; and my double star IV. 46 marked 21 :: is 20 Geminorum.

29 There is no observation by FLAMSTEED upon this star.

54 Seems to be increasing. There is an interval of 9 months between the two observations of my catalogue. Mr. BODE supposes the star to be changeable. See *Astronomisches Jahr-Buch*, 1788, page 255. And 1793, page 201.

72 and 73 There are no observations of these stars by FLAMSTEED.

78 " β appears to be of a deeper colour than it was a good " many years ago. I should now (Jan. 8, 1796) place it

" among the red, or ruddy stars; which formerly I did not " use to do."

Catalogue and Atlas.

21, 29, 72, and 73 should be out.

Atlas.

17 requires — 15′ in PD.

Notes to Leo.

" Order of magnitude May 12, 1783. α 1‴ m γ 2′ m β 2′ m $\delta\varepsilon$ $\zeta\theta\eta$ μo $\rho\nu\sigma$."

25 There is no observation of this star by FLAMSTEED; but if — 14′ 30″ in RA and + 2° 25′ 25″ in PD be applied, according to the edition of 1712, the place will then agree with 10 Sextantis. There are two very small stars near the place where 25 is put in Atlas; one of them is the star V. 63, which in my catalogue of double stars is called 25 Leonis.

26 There is an observation of this star by FLAMSTEED on page 299; but it is defective.

28 There is an observation without time on page 299, marked " *Leonis*," taken after the transit of Mars, which has probably occasioned the insertion of 28 into the British catalogue; but the observed star must have been 11 Sextantis.

38 In the observation, March 10, 1691; the number 821,66 is cast up 6 degrees too little, which produced this star; when the error is corrected, we find that the star observed was 37.

41 This is the double star in my second catalogue I. 28, and by a series of observations, it appears now that the distance between the two stars is considerably increased, since

the year 1782. I mention this circumstance here, as it will probably explain some apparent increase of brightness, that seems to have taken place in this star; for although the same quantity of light, when it is spread over more space, must appear less intense than it will do when it remains in a more confined state, it may nevertheless, by an increase of apparent magnitude, become entitled to be ranged upon a par with a brighter star.

71 FLAMSTEED never observed this star.

94 From the expressions of this catalogue, it is evident that the star is less now than it was 13 years ago.

420 of MAYER's catalogue is not visible.

Catalogue and Atlas.

25, 26, 28, 38, and 71 should be out.

WM. HERSCHEL.

Slough, near Windsor,
June 1, 1796.

XX. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1795. *By* Thomas Barker, *Esq. Communicated by* Thomas White, *Esq. F. R. S.*

Read June 16, 1796.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	Lyndon.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.
Jan.	Morn.	29,95	28,63	29,54	35½	25	30½	37	14	25	1,640
	Aftern.				36½	25½	31	44	20	28½	
Feb.	Morn.	30,17	28,49	13	44	28	34½	47	18	31	1,995
	Aftern.				45	29	35½	48	24½	34½	
Mar.	Morn.	29,82	28,15	30	45	33	40	47	20	36	2,101
	Aftern.				47	35	41½	54	32	43½	
Apr.	Morn.	29,71	28,80	31	50	41	45	51	36	42½	1,574
	Aftern.				54	41½	47½	62	41	51	
May	Morn.	29,99	29,18	69	64	42	52	59½	37	48½	0,404
	Aftern.				67	46½	55	74	48	60½	
June	Morn.	29,72	29,03	42	62½	50	55½	60	43	52	2,799
	Aftern.				67½	51	57	77	51	64	
July	Morn.	29,83	29,05	52	64½	55	58	62½	49	55	1,683
	Aftern.				66	56	59	82	55	65½	
Aug.	Morn.	29,82	29,13	49	68	58½	62½	69½	49	58	1,380
	Aftern.				71	59½	64½	84	60½	71	
Sep.	Morn.	29,99	29,18	63	68	56	62½	63	43	54½	0,057
	Aftern.				70½	56	65	78½	55	69	
Oct.	Morn.	29,70	28,67	17	62½	48	55	59½	39	50	4,536
	Aftern.				63½	50	56	69	48	58	
Nov.	Morn.	30,13	28,53	37	51	36½	44	48	24½	38½	1,850
	Aftern.				52	37	45	53½	32	43½	
Dec.	Morn.	29,96	28,87	43	51	41½	46	52	33	42½	1,382
	Aftern.				51½	42	47	54	38	46½	
											21,401

THE frost which began the latter half of December, 1794, continued long in this year, an uncommonly severe winter, for a quarter of a year; yet not without a thawing day or two now and then in January, and a greater thaw for four or five days, February 8 to 12, which took away a great part of the snow, and made a greater flood than any remembered, which did more damage to the bridges all over the kingdom than was ever known, yet without taking away all the ice and snow; the frost returned again as hard as before, and with a less break near the end of February, it continued into March. It was in general a calm frost, with vast quantities of snow coming and going; so that though it was pretty thick at times, it never lay so deep as it sometimes does. But perhaps some of the deep pits of snow and beds of ice were not entirely gone at the end of March.

After the frost broke, there came near a fortnight of wet weather, not without some snow and frost; this made the spring seed time begin very late; but when it did come, it was very favourable and quick, cool but not frosty, and the grain came up well. The beginning of the summer was dry and cool; a hot week about May 20; suddenly turned cold, with frosty mornings for some days, and then mild again. The former part of the year, both in spring and summer, was remarkably cloudy, and a great deal of cold weather and frosty mornings in May and June, and perhaps some in July; yet a few hot days at times; a week of such weather with rain at the beginning of June, brought on things very much.

A showery beginning of hay time, though not in great quantities, was some hindrance to the first got hay, but it increased the quantity of the later cut, which was also better

got; and the harvest was very fine, and as sunny as the season before had been cloudy: and it continued dry, calm, sunny, and burning much till the end of September; and then at a very suitable time, the harvest being in, and still warmth enough to make the grass grow, a wet and warm October made plenty of it against winter. The season being fine, the leaves continued long upon the trees, and the winter was very open and warm, and no considerable frost at all, but mild and warm, and the ground still green; and December was warmer and freer from frost than November. But it was a remarkably stormy winter, both for frequency and violence, doing much mischief among the shipping; and almost continual south and west winds made the passage down the Channel difficult.

XXI. *Observations on the Changes which Blood undergoes, when extravasated into the urinary Bladder, and retained for some Time in that Viscus, mixed with the Urine.* By Everard Home, *Esq. F. R. S.*

Read June 16, 1796.

The greater number of cases in physic and surgery are only important and interesting to those engaged in the study of medicine; they are not connected with general science, and therefore do not properly come under the consideration of this learned Society.

Practitioners, however, in these professions have, upon many occasions, brought to light facts of importance in the animal œconomy, which could only be discovered while the human body was labouring under disease; and those have been distinguished with a place in the Philosophical Transactions.

As every change the blood undergoes must appear an object of importance to those who study the œconomy of animals, I am induced to believe the present observations on the change produced on it by being mixed with the urine, will not be considered as wholly undeserving of notice.

I was led to pay attention to this subject from considering the following case, which came under my care.

A gentleman, seventy-one years of age, in the spring 1795, found that in making water, the urine had the appearance of

blood, and congealed into a solid mass as soon as received into the vessel. This complaint appeared to have arisen from the rupture of a vessel in one of the kidneys, for he had a pain in his loins, but none in the region of the bladder. He seemed to void no water, for the whole quantity which was expelled at any one time, amounting to about 4 ounces, formed itself into a coagulum; next day he voided bloody water, which did not coagulate. This continued for three or four days, and then went entirely off.

In the spring, 1796, he had a return of the same complaint. It came on in the evening of the 3d of April; on the 4th it was very violent; and in the afternoon there was a total suppression. A catheter was passed six or seven times; but the oval holes near the end of the instrument were always filled with coagulated blood, and no urine could be drawn off. On the 5th, a larger catheter was passed, with small round holes, less likely to have the coagulum entangled in them, but no urine came away. In the evening it was introduced again, having its cavity completely lined with a flexible gum catheter, which was withdrawn as soon as the instrument was carried to the fundus of the bladder; and in this way 4 ounces of a bloody fluid were drawn off, which on exposure coagulated.

On the morning of the 6th, a pint of bloody urine was drawn off; this operation was repeated three times in the twenty-four hours, and the same quantity was brought away each time.

On the 7th, the urine drawn off was less tinged with blood; and when it was allowed to stand, the upper part became tolerably clear. There was little change in the circumstances for six days; but on the 13th the urine drawn off was of a darker red colour, and in smaller quantity. On the 16th, the

colour was more of a light brown, and after standing some time, a whitish powder was deposited; the urine drawn off in the morning upon getting up, was nearly of the natural appearance, but that brought away in the course of the day, had a deeper tinge, and more of the white sediment. It is also to be remarked, that the sediment evidently passed off only with the last part of the urine. On the 19th, the urine was tolerably clear, and the white sediment more completely separated, and in greater quantity. In the course of the night, while lying in bed, the patient voided naturally in many different attempts, 4 ounces of water, but could not make any when up. The urine now continued clear from any tinge, but no more passed without the catheter being introduced, till the 28th, when he again made some water naturally, but could not completely empty the bladder; on the 29th, the quantity which required being drawn off was less; and by the 5th of May he made water as usual, at which time the sediment began to diminish, and gradually disappeared.

From the symptoms which have been stated, it appears that part of the blood which passed into the bladder from the kidney had remained there, and formed a coagulum, which coagulum gave a bloody tinge to the urine, and caused an inability to void it without assistance, till the coagulum was dissolved.

With a view to ascertain how far this had been the case, and discover what changes the blood undergoes when placed in such circumstances, I instituted the following experiments. They were performed by Mr. CHARLES GROVER, a very ingenious surgeon, at present house surgeon in St. George's hospital.

Experiment I. Four ounces of blood were drawn from the arm into a phial containing 4 ounces of fresh urine, and the phial was kept in the temperature of the human body; in fifteen minutes the whole mixture formed an uniform firm coagulum, and appeared wholly composed of blood.

This experiment was made to ascertain the probable time the blood would take to coagulate in the bladder.

Experiment II. Six ounces of blood were drawn from the arm into 6 ounces of fresh urine; in fifteen minutes the whole mass became one solid coagulum. In seven hours, 6 drams of clear fluid were separated from it; this was poured off, and the same quantity of fresh urine was added; after standing nine hours it was poured off; some red globules were mixed with it, but sunk to the bottom undissolved. The coagulum had fresh urine added to it three times a day, the former urine being previously poured off, and allowed to stand some hours for examination.

For the first five days the coagulum appeared to undergo little change, except becoming smaller in size, and the urine poured off from it was tolerably clear, but on standing deposited a dark cloudy sediment.

On the sixth day, the urine, when poured off from the coagulum, was of a dark red colour, and deposited a greater quantity of a dark coloured sediment, but on standing became tolerably clear.

On the ninth day, the coagulum was reduced to the size of the original quantity of blood drawn from the arm.

On the 13th day, the size of the coagulum was a good deal reduced; the urine poured off from it was still more tinged with the red globules; but when allowed to stand, the upper

part became clear, and free from the red tinge, and the sediment had the appearance of a whitish powder. From this time the quantity of white sediment increased, and the size of the coagulum diminished. In its decrease from this period the loss was from its external surface, and nearly equally all round; what remained appearing like the nucleus of the original coagulum. On the twenty-fifth day, it was of the size of a large cherry, and on the twenty-ninth it entirely disappeared. Some red globules were very distinctly seen in the sediment along with the white powder.

To see how far the changes the blood had undergone in this experiment depended on the peculiar properties of the urine, the following experiment was made, with blood and common water.

Experiment III. Six ounces of blood were drawn from the arm into 6 ounces of water. In a quarter of an hour, the whole became one solid coagulum. In twelve hours, 6 ounces of a clear water, of a bright red colour, were separated, nor did it on standing deposite any sediment.

This coagulum had fresh water added to it twice a day, and what was poured off was allowed to stand for examination.

The coagulum on the second day began to break; on the fifth had a putrid smell; and in eighteen days was almost entirely dissolved.

The water which was poured off was of a bright red colour from the beginning to the end of the experiment, in consequence of the red globules being dissolved; it had a very offensive smell, but never deposited any white sediment; the coagulating lymph dissolving from putrefaction.

As it is evident, from the result of the last experiment, that

the coagulum remaining so long undissolved in the second experiment depended upon its being mixed with the urine, I was desirous of knowing whether it was the urine incorporated with the coagulum, or that which surrounded it, which produced this effect. To determine this point I instituted the following experiment.

Experiment IV. Four ounces of blood were drawn from the arm into a cup, and allowed to coagulate. 4 ounces more were drawn into a separate cup. From each of these equal portions of coagulum, at the end of three hours, 1 ounce of serum was separated, and poured off. To one of them fresh urine was added; to the other common water. The urine and water were changed night and morning.

The water was tinged of a bright red colour throughout the whole experiment, and deposited no sediment. On the eighth day the coagulum was rather looser in its texture. On the thirteenth day it began to break, and by the twentieth day it was nearly dissolved. The progress corresponding with that of the coagulum in *Experiment* III.

The urine the second day of the experiment was clear, but the bottom of the bason was covered with red globules, undissolved.

On the fifth day, the urine poured off was tinged of a bright red colour, similar to the water taken from the other coagulum; and after standing some hours a white sediment was deposited.

On the thirteenth day it was looser in texture, and more dissolved than the coagulum in the water. It continued to tinge the urine of a bright red colour, and what was poured off deposited a white sediment in greater quantity. On the

eighteenth, the coagulum was nearly dissolved; so that the coagulum immersed in the urine dissolved two days sooner than that in the water.

From this experiment we find, that it was the urine incorporated with the coagulum in *Experiment* II. that prevented the red globules from dissolving, and preserved the coagulum for so long a time, since these effects were not produced by urine while simply surrounding the coagulum.

If we compare *Experiment* II. with the result of the case, they agree so entirely, that it leaves no doubt of the process carried on in the bladder being similar to that which took place out of the body. The patient was unable to make water for twenty-four days, although the passages readily admitted, during the whole of that time, an uncommonly large instrument, which could not have been the case had there been any obstruction in them; for six days more he voided it with difficulty, but afterwards made water very well.

The coagulum out of the body was reduced in twenty-five days to the size of a cherry, and in four days more it was completely dissolved.

The patient's urine became darker, from the red globules mixing with it, in nine days. In the experiment this took place in five days.

The white sediment was first observed, in both instances, about the twelfth day; it continued to be deposited till the patient got well, and to the end of the experiment.

That the blood is capable of uniting with a quantity of urine equal to itself, so as to form a firm coagulum; that the red globules do not dissolve in a coagulum so formed; that an admixture of urine prevents the blood from becoming putrid;

and that the coagulating lymph breaks down into parts almost resembling a soft powder, are facts which I believe to be new; —they may, however, have been before ascertained, although I have not been acquainted with them.

They are certainly not generally known, and one object of the present paper is to communicate them to others.

These facts, considered abstractedly, may not appear of much importance; but when compared with what takes place in the living body, and found to agree with the process the blood undergoes in the urinary bladder, they become of no small value, since they enable us to account for the symptoms that occur in that disease, and lead to the most simple and effectual mode of relieving them.

XXII. *On the Fructification of the submersed Algæ.* By Mr. Corrêa de Serra, *F. R. S.*

Read June 16, 1796.

The light which the prevailing spirit of inquiry and observation has thrown on the means of reproduction allowed by nature to vegetable beings, is not yet equally diffused over all of them. Those whose simpler organization seems, when examined, to want some of the parts which we are accustomed to consider as essential to generation, continue to the present moment more or less involved in darkness; and their fecundation, and means of reproduction, are still objects of doubt and inquiry. Amongst them the Fuci, Ceramiums, Ulvæ, Confervæ, all submersed algæ, are perhaps in the number of the less illustrated. It is probable that their peculiar way of living, which requires from nature a particular modification in the parts destined to reproduce them, as well as in the means of performing this operation, has been the principal cause of the perplexity of naturalists on this subject. They have either sought for things in their ordinary form, which nature furnishes to these plants under a different one, adapted to their circumstances; or they have thought that she deviates from her usual ways, when she only makes use of her stubborn versatility, enforcing the execution of her general plan, by the means which at first sight seem to make her deviate from it. In the present memoir I shall

endeavour to ascertain what parts of these plants perform the sexual functions: and, in order to be clear, I will first relate in a few words, what has been observed and believed on this point, and proceed afterwards to the exposition of the opinions which observation and the strictest analogy induce me to hold on this subject.

REAUMUR was the first naturalist who bestowed a proper attention upon the fructification of the Fuci. Two elaborate memoirs of this great man are to be found, in the Parisian Transactions for the years 1711 and 1712, in which he endeavours to persuade us, that the vesiculæ filled with small grains are the female part of the fructification, in the fuci, and the filamentous hairs, which are found in different parts of the frons, the male organs. He examined eleven species of fuci, in eight of which he found grains, and only in six the filamentous hairs. It is unlucky for his opinion, that these hairs have no antheræ, and still more unlucky, that their existence has no relation at all to that of the vesicles which bear the grains, for they are persistent through all the life of the plant, without any remarkable alteration. Their situation besides is very unfit for the fecundation of the grains, except in the Fucus elongatus. Notwithstanding the weight of these objections, which he did not conceal, this otherwise sensible naturalist tried to the last to support by hypotheses, what he could not fairly prove by observations. His great name, joined to the general ascendency which the sexual system gained a little after all over Europe, gave nevertheless a common currency to his opinion; and it was received, though in a wavering manner, by LINNÆUS himself, and, what is more surprising, by the last of the JUSSIEUS. These great men indeed gave it as the prevail-

ing opinion of the day, not confirmed by any sanction of theirs. This was not the case of less profound botanists; for they, as the fashion then was, attempted to see stamina and antheræ, like the common ones, wherever they had not been observed before. I deem it unnecessary to stop a moment to consider the multitude of supposed stamina, which DONATI, GRISELINI, and others, imagined they had found in Fuci, and Ulvæ, because it is at present clearly evinced that these fancied stamina are only organs of nutrition.

Cooler observation and reflection exploded at length all these dreams; but GMELIN, and GÆRTNER, the two greater among the naturalists who followed a different way of thinking, went perhaps too far on the opposite side.

GMELIN, convinced both by reason and observation of the inutility of the REAUMURIAN antheræ, and writing at a time when the recent publications on the hydræ, and on the aphides, had made it fashionable to find examples of multiplication of organized bodies without fecundation, determined to consider these plants as in the same predicament. Every one may see, in his *Historia Fucorum*, the elaborate discussion by which he endeavours to establish his opinion. It dazzles at first sight, but, on a candid examination, all his arguments, when deprived of the apparatus of science which accompanies them, may be reduced to the following; namely, that since the supposed male organs are not such in reality, and no others are to be found, the small grains which act as seeds are prolific, without receiving external fecundation. We shall see as we proceed, how groundless is the supposition on which this argument rests.

GÆRTNER, by far a deeper naturalist than the preceding, and keeping more closely to the ways of nature, would, no

doubt, have given us the true and simple account of the fructification of the submersed algæ, if the specious ADANSONIAN theory of aphrodite plants, and his own ideas of the perfect seed, had not led him (according to my opinion) astray. The grains of the Ceramiums and Ulvæ, and the lateral internodia of the Confervæ, he excludes from the number of seeds, and believes them to be gems of a particular kind, which he calls *gongyli*, or *gemmæ carpomorphæ*, consequently not standing in need of fecundation. The grains of the fuci he judges to be true seeds; but, in this case, he believes that the uterus performs the male functions, and that the plants, in respect to their fecundation, are *aphroditæ*, having only the *apparatus fœmineus, et intimam utriusque sexus sub specie singuli copulam.* Both he and GMELIN, forced by phænomena which they could not help observing, have been in some moments very near to what I conceive to be the truth, but have sacrificed it to preconceived opinions.

In the last year two English botanists, to whom science stands indebted for many excellent descriptions and figures of fuci, Major VELLEY, and Mr. STACKHOUSE, treated this same matter, the first at large, the second occasionally. Both have stated, with great ingenuity and candour, the many objections which attend the existing systems, and both declared themselves not fully satisfied with the present state of our knowledge on this subject. Mr. STACKHOUSE, indeed, seems to cherish hopes of future discoveries of the male organs, in what he calls the concealed fibrous fructification, the antheræ not seeming necessary to him, nor the farinaceous pollen.* Perfectly agreeing with him, in what respects the needlessness of a farinaceous pollen, I cannot accede to the other parts of

* *Nereis Britannica*, in the preface, and page 30.

his opinion. A membranaceous loculament, containing the pollen, is the only necessary part of the male apparatus in plants; the filaments and the fibrous texture are only the pedicles of it, and very far from being necessary, as the sessile antheræ of numberless fructifications clearly prove. If a fibrous concealed structure could be esteemed of any use, it was already found by GMELIN, in the seed-vessels of the true fuci,* and elegantly described by Major VELLEY, in the *Fucus Vesiculosus*, and by Mr. STACKHOUSE himself, in the *Fucus Siliquosus;* but, even when magnified, it offers nothing more than simple tubular vessels, with frequent anastomoses, very remote indeed from the nature of a male apparatus.

Having stated the leading systems on the fructification of submersed algæ, I will next submit to this Society such opinions as the phænomena I have observed induce me to have about it.

All these plants are furnished with grains, which are a temporary production, and, by their falling, give rise to new individuals of the same species. In the true fuci, they are contained in an uterus, which has a temporary existence, and for their sake only, where they have a placentation, and are covered by a testa, or coat of their own. Nobody doubts that they are true seeds. The Ceramiums and Ulvæ have the same grains, as means of reproduction; and the confervæ also have them, though of a different shape. What then can be the reasons why these last are to be considered as *gongyli* and *gemmæ carpomorphæ?*

The only arguments adduced to deprive them of the nature of seeds are the three following.

1st. The grains of the Ulvæ and Ceramiums are solitary, not

* *Hist. Fucorum*, pag. 25, *et seq.*

contained in a proper uterus, consequently without a placentation. They are, says GÆRTNER, part of the medulla of their mother, and their skin is part of the maternal one.*

2d. They do not, in germinating, leave any coat behind.†

3d. In the confervæ, whose grains have some likeness to fresh internodia, two or more of them very often coalesce, but give rise to only one individual.‡

All these reasons require to be candidly discussed; and I hope the result of the investigation will afford us many additional motives to believe them to be true seeds.

The first of these objections cannot stand the test of close examination. The grains of the Ceramiums (like those of the true Fuci) fall at a proper period, which GÆRTNER calls *senium*, but which others will call maturity. If gently squeezed, they come forth from the little cavity where they are formed, and which they must leave when ripe. They come forth whole, and disengaged from the mother, and from every part of the frons; they have therefore a skin of their own. They are contained in a small uterus, proportionate to their size, which is of a temporary existence, and for them alone; where they are no doubt affixed by some placentation, from which, when they come to maturity, they are disengaged and fall. If we add to these considerations, that of their existing there enveloped in a soft juicy substance, all their difference from the seeds of the true fuci wholly disappears; § and a strong probability arises, that GÆRTNER's

* *De Fructibus*, &c. pag. xvi. *et seq.* † Ibidem, pag. xi.

‡ Ibidem, pag. xviii.

§ I have made mention of the Ceramiums in this paper only to follow GÆRTNER through his objections. This genus, first made by DONATI, adopted by ADANSON, and GÆRTNER, has in reality scarcely any difference from the fuci.

observations were made on dry specimens, as well as with a mind not wholly impartial to his preconceived theories.

The second reason is of a more specious nature, and requires serious attention. Animals are divided into oviparous and viviparous, and a generally received comparison points out the seeds as the eggs in plants, and the gems as correspondent to living born fœtuses. We cannot conceive, says GÆRTNER, an egg where the animal, when coming forth, does not leave the shell behind; and, in the same manner, we cannot conceive a seed where the coats are not left behind in the germination. The grains of the Ulvæ, Ceramiums, &c. according to him, do not leave any coat when they germinate, and are consequently *gemmæ carpomorphæ*. Every candid naturalist will easily acknowledge, that we are not possessed of observations sufficiently decisive to enable us to speak dogmatically, on phænomena so little obvious as the germination of these grains. But I will not contest the fact, I will only examine the principle.

This general rule, of judging whether these grains be seeds or gems, by their leaving their coats in the germination, or not, is contradicted by nature, both in the instance of gems, and in that of eggs. All gems, properly so called, throw off their scaly hybernacula in the act of germinating. On the contrary, the eggs of frogs and toads leave no coat at all in their hatching, because they are possessed of none. Their very viscous albumen answers, in such an element as water, all the purposes the testa accomplishes in other eggs. Allowing GÆRTNER the exactness of his observation concerning our plants, the analogy between these submersed algæ and the aquatic oviparous quadrupeds would be striking, since both those plants and these animals are capable, from their struc-

ture, of being nourished by absorption; their embryos are hatched in the same element, and equally surrounded by a tenacious mucous substance, without any exterior coats. If the spawn of frogs be eggs, the grains of these plants must be seeds.

The third objection, very far from being an urgent one, is, I am persuaded, a capital reason to believe that the seeming lateral internodia of the Confervæ, since they are capable of cohering two or more together, and produce only one individual, are true seeds, and not gems. The coherence of two living embryos, whether gems or seeds, may form monsters, but it is equally impossible, in both cases, that perfect individuals should regularly be formed by such coalition. Observation daily shows, that of two or more neighbouring gems or seeds, one may thrive by rendering the other abortive; but, in this case, gems never cohere, the abortive one falls. In seeds, on the contrary, not only the abortive coheres to the thriving one; but this abortion happens oftener in the several species of plants, in proportion as the seeds, by their situation, are apt to cohere. In some genera it is even a regular proceeding of nature, as in the Dalea, Lagœcia, Hasselquistia, Sapindus, Ornitrophe, &c.

These objections having been I hope satisfactorily answered, I do not hesitate to consider these grains of the submersed algæ to be, what obviously they seem, their effective seeds. The figure, formation, and temporary fall of these bodies, would never have left any room for the above doubts, if their fecundation had been easily accounted for. This point we must now proceed to investigate, and examine whether the mucous

substance which surrounds these seeds can be considered as true pollen.

Pollen is by its nature immiscible with water, and specifically lighter than that fluid. In the aquatic plants, which have a farinaceous pollen, the buds of the flowers emerge from the surface of the water, and the fecundation is performed in the open air. The phænomena attending the blossom of the Potamogetones, Myriophylla, Vallisneria, &c. are too well known to require a particular mention. In some aquatic plants, whose flowers have not the faculty of emerging from water in the period of fructification, but still are endowed with farinaceous pollen, nature has taken every precaution to defend it from that element. The flower of the Zostera is situated, and its fecundation happens, in the interior cavity of the stem, which opens itself afterwards to let loose the fecundated seeds. The concave bases of the leaves in the Isoetes, closely adhering to each other, and perhaps more so in the act of fecundation, forbid the entrance of water to the minute flowers situated within them. In the Pilularia, and Marsilea, whose flowers are exposed to inundations, the fecundation is performed in perfectly closed vessels. Even in plants living in the air, nature employs numberless well known contrivances, to shelter the farinaceous pollen from the contact of water in rainy seasons.

The pollen, to be active in fecundation, needs not always be farinaceous. In most Apocyneæ it is rather a fluid; in the Orchideæ it is an aggregate of solid parts, of a ceraceous appearance; in some Contortæ it is found in a solid or rather viscous state. In the Pilularia, and Marsilea, the particles of the pollen are kept in small bags of a mucous

substance, compared by BERNARD DE JUSSIEU to dissolved gum. The original state in which it is found in every flower, before the act of fecundation, is that of a mucus, but it is perfectly active even in that state, since its particles, after becoming a little dry, if put into certain fluids, are seen, by the help of the microscope, acting in the same manner as when in the state of perfect farina.

The plants whose fructification lies unsheltered under water are very few in number; and such of them as have been hitherto thoroughly examined, (the Ceratophyllum and the Chara), have antheræ furnished with mucous pollen, not bursting in the fecundation. From the time of DILLENIUS it has been observed, that the submersed antheræ of the Ceratophyllum never burst, but are found whole, though the seeds be ripe.* If squeezed, they shed a soft and pulpous matter, like that which is found in unripe antheræ. DILLENIUS suspected that the fructification of the Chara being equally submersed, its antheræ and pollen would be of the same nature with the preceding, and observations have fully confirmed the conjectures of this great naturalist. The antheræ of the Chara do not burst in fecundation; its pollen is mucous; the germen has no pistillum, and is probably fecundated through its receptaculum, as there exists in each internodium, according to modern observations,† a chain of vessels which twist round the antheræ and the fruit, and in which a circulation of humours is visible, at least in the period of fructification.

If pollen therefore, under the shape of farina, be unfit for fecundation in the water; if nature has taken a particular care to guard this operation from the presence of that element; if

* *Plantæ Gissenses*, pag. 91.

† CORTI, *Osserv. Microscop.* Lucca, 1774.

pollen can exist in an active state under a mucous appearance; and if the antheræ of perfectly submersed flowers are nothing else than closed vessels filled with mucous pollen; what doubt can we entertain, that the mucilaginous vesicles of the submersed Algæ (which contain also their seeds) are antheræ, and very appropriate to the nature and situation of these plants?

An observation made by GLEICHEN shews more clearly the propriety of such a fructification. The pollen of any flower, when put into water, in a very short time begins to move, and its particles agitate themselves in every direction, perfectly resembling the most lively animalcula. Their activity in this state lasts some time; but, if the least quantity of salt be put into the liquor, death quickly ensues, from which they never more recover.* This inclosed mucilaginous fructification was therefore the only one which could ensure existence to vegetables living chiefly in sea-water, with which their mucus is found to be immiscible.†

A still more urgent consideration will, I hope, determine those who may hesitate to consider the mucus of these plants as pollen, and the vesicles which contain it as antheræ. The parts of fructification, in all plants, are temporary, and their existence is relative to their particular functions, and to each other. The moment the fecundation happens is a moment of crisis: henceforth the fecundated parts proceed to grow and perfect themselves, the fecundating ones change and decay. This is a general law of nature, to which we know no exception, nor can any be easily conceived to exist. We must remark, that there is an epoch when the mucous substance in

* GLEICHEN *Observ. Microscop.* pag. 32. † GMELIN *Hist. Fucorum*, pag. 27.

the vesicles of the fuci suffers a material alteration, but the grains proceed to their perfection. In the *Fucus vesiculosus* this change of colour and consistency in the mucus is evident to common sight. It is still more evident in the *Fucus selaginoideus*, where the temporary bright and vivid colour of the mucus, followed by a prompt decay after that period, has struck even those naturalists who most decidedly opposed the existence of male parts in these plants;* and I am confident, from the steadiness with which nature adheres to her general plans, that proper observations will demonstrate the same in every species of these submersed algæ, and confirm what the foregoing analogies induce me to think, *viz.*

That the vesicles of all these plants, whatever be their shape, if containing grains and mucus, are to be considered as hermaphrodite flowers; the grains they contain as their seeds, and the mucous substance as their pollen.

* Gærtner, pag. xxxii.

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ERRATA.

PART I.

P. 143, l. 3, *for* affraïant, *read* effraïant.

Note from HENRY BROUGHAM, Jun. Esq. author of the paper on the inflection, reflection, and colours of light. See page 227, &c.

" Owing to an error which crept into the integral calculus by which the problems on " the trajectory of light were resolved, two of these solutions are erroneous, and must be " corrected thus: 1. When the bending force is inversely as the distance, the curves to be " squared are, a conic hyperbola, and a logarithmic, $y^2 = \frac{1}{l\frac{a}{x}}$. The trajectory, there- " fore, cannot be found in finite terms; its equation is $\dot{y}^2\, l\frac{a}{x} = \dot{x}^2$; and the sub- " tangent is to the subnormal as 1 to $l\frac{a}{x}$. 2. When the bending force is inversely as " the square of the distance, the curves to be squared are a cubic hyperbola, $y = \frac{1}{x^2}$, " and a cubic conchoid, $y^2 = \frac{x}{a-x}$; therefore the equation to the trajectory is " $(a-x)\,\dot{y}^2 = x\,\dot{x}^2$, which belongs to a *cycloid*, the radius of whose generating circle is " a. In general, if the force be inversely as the mth power of the distance, the equation " of the trajectory will be $(a^{m-1} - x^{m-1})\,\dot{y}^2 = x^{m-1}\,\dot{x}^2$, which agrees also with the " first case, where m being $= 1$, $\underline{a^{m-1}}$, may be esteemed the hyperbolic logarithm " of a."

H. BROUGHAM.

Edinburgh,
July 2, 1796.